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# PROCEEDINGS

OF THE

# CAMBRIDGE PHILOSOPHICAL SOCIETY.

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## VOLUME III.

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# PROCEEDINGS

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# Cambridge Philosophical Society.

*October* 23, 1876.

PROF. J. CLERK MAXWELL, F.R.S., PRESIDENT, IN THE CHAIR.

The following Communication was made to the Society by

Mr J. H. PARKER, C.B., *On the Forum and Colosseum at Rome.*

Mr Parker gave a detailed account of the Forum Romanum and Colosseum, as they are now laid open to view by recent excavations. He began by observing that the received interpretations of those passages in the classics which relate to the historical topography of Rome rest entirely upon what are called the *Roman traditions,* dating only from the sixteenth century, and often far from the truth. By the aid of lime-light he next exhibited a series of views shewing first the Forum as it appeared in 1650 (from an engraving) with 30 ft. of earth upon it, and the same site as at present excavated; and then he took the audience an imaginary walk with him from the Capitol down the Forum, noting in succession the three columns of the temple of *Saturn* and the aerarium (treasury) behind it—the temple of *Concord* and the seven columns of the temple of *Vespasian*—the arch of Septimius Severus,—the column of Phocas ('nameless column with a buried base')—two wondrous screen-walls with bas-reliefs of the beginning of the second century, representing (1) a boar, ram, and bull in procession for the *suovetaurilia*, and (2) a file of citizens bringing the records of their state-debts to be burnt, according to a generous decree of the Emperor Hadrian. In the Via Sacra he shewed the temple of Antoninus and Faustina as it was till recently, and also in its present condition now that the monolithic columns of cipollino marble, 46 ft. high, can once more be seen in all their beauty—the temple of Romulus (the son of Maxentius)—the Cloaca Maxima—and the arches of Titus and

Constantine, on the latter of which a plan of the Forum is visible, and also several bas-reliefs of earlier date and finer style; these had been probably removed from the arch at the entrance of Trajan's Forum.

After a short discussion with Mr Burn upon the aerarium, with reference to the temple of Saturn, Mr Parker proceeded to give a history of the Colosseum, and insisted that recent excavations prove that we have in the existing building old *tufa* walls of the first amphitheatre of Scaurus (aedile 58 B. C.), over which is brick-work of the time of Nero; and to these Vespasian and Titus added the magnificent stone front and double corridors. In one of the photographs of the upper part could be seen the corbels and holes for the masts, 20 ft. high, which were to support the ropes of the awning; these were strong enough to bear an elephant and his rider.

In the old tufa walls are vertical grooves for lifts to send up the wild animals in cages on the stage or arena above, through trap-doors; and behind the outer wall, nearly under the *podium* of the lower gallery, are dens for the wild beasts. It is actually said that on some occasions a hundred lions leapt on to the stage at once. Down the centre of the building is a wide passage called the gulf, on the floor of which is an ancient framework in wood, that looks as if it had been burnt, and which has all the appearance of what is called a *cradle* in dockyards, for vessels to stand upon when not in use. On each side of this central passage are remains of two canals of water, supplied by the aqueducts for the keels of the vessels, at the time of naval fights. On one occasion it is recorded that these canals were filled with wine instead of water. Here in one and the same day the Emperor Commodus is said to have taken part in a wrestling match, which was succeeded by a fight with wild beasts, this again by a sea-fight, and fourthly by his orders the arena was cleared and then spread for a banquet, which followed immediately. The scenes must have been prepared below, and sent up in the central passage to the arena above. One of the photographs was a representation of the Tarpeian rock, from the top of which culprits were thrown to the bottom of the gulf. The rock was 50 ft. high, and the gulf 21 ft. deep, so that the culprits were cast down 71 ft., and the scene was witnessed by eighty thousand people.

All these very interesting substructures are at present under water to the depth of ten feet, and a great drain is to be made to carry off the water to the Tiber. The upper part of the building is the same as it has been for many years, and is well known from engravings; but all these substructures are an entirely new discovery, and the photographs exhibited are now the only record of them that is visible.

Photographs of amphitheatres at Capua and Pozzuoli (Puteoli) were also exhibited for comparison and illustration, and after a few questions and criticisms by the President, Mr Jackson, and others, the meeting closed with a vote of thanks to Mr Parker.

*October* 30, 1876.

## (ANNUAL GENERAL MEETING).

PROF. C. C. BABINGTON, F.R.S., VICE-PRESIDENT, IN THE CHAIR.

The Annual Election of Officers, and of Members of Council, to fill the vacancies caused by retirement or otherwise, took place.

The following Communication was made to the Society by

Mr G. T. BETTANY, B.A., *On the Primary Elements of the Skull.*

This communication contains an account of some of the most recent conclusions adopted by Prof. Parker, the distinguished honorary member of this Society, to whom science owes so large a proportion of our knowledge of the subject. These views will be more fully detailed in a forthcoming work on *The Morphology of the Skull* by Prof. Parker and myself. The question to be considered this evening is not the segmentation of the skull or the comparison of its segments with those of the body, but what parts in it are axial and what appendicular, whether indeed the axis of the body ceases in the middle of the base of the skull, and the rest of it, containing the fore part of the brain, has to be walled in by the help of other structures. The rapidity with which important phases of development are passed through in higher vertebrates, and the extreme minuteness of the structures, cause much difficulty in arriving at the truth; that remarkable event in early growth, the *mesocephalic flexure*, is one of the most puzzling in itself and in its consequences. Again, in all the principal types, we appear to be brought face to face with the fact that every form which survives at present has become in important respects highly specialised in structure with regard to its conditions of life, even though it may have many features which place it on the whole low in the scale. Much apparent lowliness may also be caused by the fact that in some cases degeneration of structure has afforded a more complete adaptation to a particular kind of life: most parasites illustrate this. The inference I wish to draw is that the development of no one type is to be taken as an absolute guide to principles.

Professors Huxley and Parker have for some years been led to view the *trabeculæ cranii*, the primary elements underlying the base of the fore part of the brain, as not axial in character, but as comparable in effect to the visceral arches, or mandibular, hyoidean, and branchial series. In his *Anatomy of Vertebrated Animals*, 1871, p. 77, this view is alluded to by Prof. Huxley as a hypothesis or speculation, which must not be placed on the same footing as the doctrine of the segmentation of the skull. But without, as far as I am able to discover, any full and formal exposition of the evidence, this hypothesis has gradually been elevated almost to the rank of a doctrine by the two writers named above, until in a paper on *Ceratodus* by Prof. Huxley, in the Proceedings of the Zoological Society for 1876, Part I., the trabeculæ are classed without reserve as pleural elements. The following appear to be some of the chief considerations in favour of the view that the trabeculæ are visceral arches:

1. Their downbent condition during the mesocephalic flexure, when they necessarily appear more or less parallel to the visceral arches behind.
2. Their transitory distinctness in several types from the parachordal elements which lay the foundation of the hinder part of the skull.
3. The fact that the notochord does not extend beyond the pituitary body which is developed between the hinder regions of the trabeculæ.
4. Their anterior junction in the ethmoidal region has some analogy with the ventral junction or union between each pair of visceral arches.
5. Their more or less curved shape at first.
6. The distribution of the orbitonasal division of the trigeminal nerve, which presents some features of resemblance to the distribution of nerves on the visceral arches.

Against the weight of these considerations it may be alleged, that the tissue beneath the fore brain before the mesocephalic flexure has the same claim to be called axial as the tissue beneath the hinder part of the brain; that the fact of the mesocephalic flexure carrying it into a different position does not make this tissue other than axial; and that it resumes the axial position very early. The trabeculæ in almost all cases become united with the parachordals before uniting with each other, constituting a single bar on each side in the base of the brain. The significance of the notochord and pituitary body is unknown; but in several instances (Axolotl, Dog-fish) the trabeculæ acquire a distinct parachordal region which may be proportionately very extensive. As development advances, the side walls of the brain-case chondrify continuously with the trabeculæ just as the occipital ring is formed

in continuity with the parachordals: and in these lateral walls nerve foramina occur in precisely the same way. In fact every relation of the trabeculæ proper is neural. Accepting the morphological identity of the trabeculæ with the parachordals, they may either be regarded as truly axial, or as the basal parts of proper neural arches; the latter view would seem to cause great difficulty in accounting for the basioccipital, basisphenoid, and presphenoid bones. Assuming the trabeculæ to be axial or neural elements, one might expect more or less definite appendicular pieces to be found in relation to them. The mandibular arches have a primary relation to the hinder part of the trabeculæ; while the antorbital or lateral ethmoidal (with its frequently distinct antorbital cartilage), the cornual and the prenasal regions offer developments which may with some reason be considered as appendicular. On this view the orbitonasal nerve would be accounted for: the trigeminal nerve might be regarded as a nerve of several segments like the pneumogastric: its branches being distributed along the cornua, over the ethmoidal region on either side, and on the palatopterygoid and the mandibular tracts. The relations of the sense organs to the axial parts are very strange if the trabeculæ are visceral arches; while they seem more intelligible when they are regarded as paraxial elements, the olfactory organs situated between the cornual and antorbital regions, the eyes between the antorbital and mandibular, the ears between the mandibular and hyoid.

In conclusion, I would submit that the doctrine of the trabeculæ being visceral arches presents so many difficulties, that it ought not be adopted without the strongest evidence in its favour: the counter-view is urged as more natural and simple, as well as more in accordance with facts.

*November* 6, 1876.

PROF. CLERK MAXWELL, F.R.S., PRESIDENT, IN THE CHAIR.

The following Communications were made to the Society by

(1) Mr J. W. L. GLAISHER, M.A., F.R.S., *On a Formula of Cauchy's for the Evaluation of a class of Definite Integrals.*

In his Memoir "sur une formule relative à la détermination des intégrales simples prises entre les limites 0 et ∞ de la variable" (*Exercices des Mathématiques*, t. I. pp. 54—56, 1826) Cauchy has given his now well-known evaluation of the integral

$$\int_0^\infty x^{2n} f\left(x - \frac{1}{x}\right) dx,$$

$f(x)$ being supposed to be an even function of $x$, viz. denoting this integral by $B_{2n}$, and putting

$$A_{2n} = \int_0^\infty x^{2n} f(x)\,dx,$$

then the formula is

$$B_{2n} = A_0 + \frac{(n+1)n}{1.2} A_2 + \frac{(n+2)(n+1)n(n-1)}{1.2.3.4} A_4 \dots$$
$$+ \frac{2n-1}{1} A_{2n-2} + A_{2n} \dots\dots\dots (1);$$

but it is curious that neither Cauchy, nor, as far as I know, any of those who have quoted his result, seem to have noticed the corresponding formula for the case when the arbitrary function is uneven; which may be thus enunciated.

If $\phi(x)$ denote an uneven function of $x$, and if

$$P_{2n-1} = \int_0^\infty x^{2n-1} \phi(x)\,dx, \quad Q_{2n-1} = \int_0^\infty x^{2n-1} \phi\left(x - \frac{1}{x}\right) dx,$$

then

$$Q_{2n-1} = nP_1 + \frac{(n+1)n(n-1)}{1.2.3} P_3$$
$$+ \frac{(n+2)(n+1)n(n-1)(n-2)}{1.2.3.4.5} P_5 \dots + \frac{2n-2}{1} P_{2n-3} + P_{2n-1} \dots (2).$$

§ 2. This formula can be proved exactly as Cauchy proved (1); for using $\phi(x)$ to denote an uneven function of $x$ and $f(x)$ an even function of $x$; we find by taking $x = \frac{1}{x'}$, that

$$\int_0^\infty \phi\left(x - \frac{1}{x}\right) dx = -\int_0^\infty \phi\left(x - \frac{1}{x}\right) \frac{dx}{x^2} \dots\dots\dots\dots (3),$$

and that generally

$$\int_0^\infty x^{2n-1} \phi\left(x - \frac{1}{x}\right) dx = -\int_0^\infty x^{-2n-1} \phi\left(x - \frac{1}{x}\right) dx \dots\dots (4);$$

while by taking $x - \frac{1}{x} = x'$ so that to the limits $\infty$ and $0$ of $x$ correspond the limits $\infty$ and $-\infty$ of $x'$, we have

$$\int_0^\infty \left(x + \frac{1}{x}\right) f\left(x - \frac{1}{x}\right) \frac{dx}{x} = \int_{-\infty}^\infty f(x)\,dx = 2\int_0^\infty f(x)\,dx \dots\dots (5).$$

Now in the equation

$$\sin 2n\theta = 2n\cos\theta\left\{\sin\theta - \frac{n^2-1^2}{3\,!}\,2^2\sin^3\theta + \frac{(n^2-1^2)(n^2-2^2)}{5\,!}\,2^4\sin^5\theta - \&c.\right\}$$

put

$$2\cos\theta = x + \frac{1}{x},\quad 2i\sin\theta = x - \frac{1}{x},$$

and we obtain

$$x^{2n} - \frac{1}{x^{2n}} = \left(x+\frac{1}{x}\right)\left\{n\left(x-\frac{1}{x}\right) + \frac{(n+1)n(n-1)}{3\,!}\left(x-\frac{1}{x}\right)^3 + \frac{(n+2)(n+1)n(n-1)(n-2)}{5\,!}\left(x-\frac{1}{x}\right)^5 + \&c.\right\}\ldots\ldots(6),$$

which, after multiplication throughout by $\phi\left(x-\frac{1}{x}\right)\frac{dx}{x}$ and integration between the limits 0 and $\infty$, gives (2), since

$$\int_0^\infty \left(x^{2n}-\frac{1}{x^{2n}}\right)\phi\left(x-\frac{1}{x}\right)\frac{dx}{x} = \int_0^\infty (x^{2n-1} - x^{-2n-1})\,\phi\left(x-\frac{1}{x}\right)dx$$

$$= 2\int_0^\infty x^{2n-1}\phi\left(x-\frac{1}{x}\right)dx, \text{ from (4)},$$

while the right-hand side assumes the form in (2) in virtue of (5).

§ 3. The proof may be presented in a somewhat different form as follows:—

First, ($f$ being an even function as before), show that

$$\int_0^\infty f\left(x-\frac{1}{x}\right)dx = \int_0^\infty f(x)\,dx \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\;(7).$$

This is readily effected, for if

$$u = \int_0^\infty f\left(x-\frac{a}{x}\right)dx$$

($a$ positive), then $\frac{du}{da} = -\int_0^\infty f'\left(x-\frac{a}{x}\right)\frac{dx}{x}$: transform this integral by taking $x = \frac{a}{x'}$, and we have

$$\frac{du}{da} = -\int_0^\infty f'\left(\frac{a}{x}-x\right)\frac{dx}{x} = \int_0^\infty f'\left(x-\frac{a}{x}\right)\frac{dx}{x}$$

since $f'$ is an uneven function. Thus $\frac{du}{da} = -\frac{du}{da}$ and therefore $= 0$, so that $u$ is independent of $a$, and we have

$$\int_0^\infty f\left(x - \frac{a}{x}\right)\frac{dx}{x} = \int_0^\infty f(x)\,dx \text{ .................. } (8),$$

of which (7) is the case when $a = 1$.

It then follows that

$$\int_0^\infty f(x)\,dx = \int_0^\infty f\left(x - \frac{1}{x}\right)dx = \int_0^\infty f\left(x - \frac{1}{x}\right)\frac{dx}{x^2}$$

$$= \tfrac{1}{2}\int_0^\infty \left(1 + \frac{1}{x^2}\right) f\left(x - \frac{1}{x}\right)dx,$$

which is (5), and the proof proceeds as before.

§ 4. But having proved (7), the more interesting procedure is to divide the equation (6) by $x + x^{-1}$ previous to integration, viz. we have

$$x^{2n-1} - x^{2n-3} \ldots - x^{-2n+1} = n\left(x - \frac{1}{x}\right) + \frac{(n+1)n(n-1)}{3!}\left(x - \frac{1}{x}\right)^3 + \&\text{c.}$$

and on multiplication by $\phi(x - x^{-1})$ and integration the right-hand side becomes $nP_1 + \&c.$, *i.e.* the right-hand side of (2), while

$$\int_0^\infty (x^{2n-1} - x^{2n-3} + x^{2n-5} \ldots + x^{-2n+3} - x^{-2n+1})\,\phi\left(x - \frac{1}{x}\right)dx$$

$$= \int_0^\infty x^{2n-1}\phi\left(x - \frac{1}{x}\right)dx,$$

all the other terms destroying one another, since by (4)

$$\int_0^\infty x^{2n-3}\phi\left(x - \frac{1}{x}\right)dx = -\int_0^\infty x^{-2n+1}\phi\left(x - \frac{1}{x}\right)dx$$

$$\int_0^\infty x^{2n-5}\phi\left(x - \frac{1}{x}\right)dx = -\int_0^\infty x^{-2n+3}\phi\left(x - \frac{1}{x}\right)dx,$$

&c. = &c.

Thus, regarding (7) as the fundamental theorem, we see that we obtain a formula for

$$\int_0^\infty x^{2n-1}\phi\left(x - \frac{1}{x}\right)dx,$$

(although $x^{2n-1}$ is not a function of $x - x^{-1}$), because $x^{2n-1} - x^{2n-3} \dots - x^{-2n+1}$ is a function of $x - x^{-1}$, and all the terms except the first cancel one another by the integration.

Similar remarks of course apply to Cauchy's formula (1).

§ 5. The formula (2) can be deduced from Boole's general theorem

$$\int_{-\infty}^{\infty} \phi(x) f\left(x - \frac{a}{x}\right) dx = \int_{-\infty}^{\infty} dv f(v)\, \Theta[\phi(x)] \frac{x}{vx - x^2 + a},$$

(*Phil. Trans.* 1857, p. 782) in exactly the same way as Boole himself obtained Cauchy's formula (1)[1].

In Boole's formulæ the quantity subject to the functional sign is $x - ax^{-1}$ ($a$ positive), and it is worth noticing that (1) and (2) can readily be so transformed as to assume the more general form.

Take, for example, (2), replace $\phi(x)$ by $\phi(ax)$, and transform the left-hand integral by taking $x = x' : b$; thus

$$\int_0^{\infty} \frac{x^{2n-1}}{b^{2n}} \phi\left(a\frac{x}{b} - a\frac{b}{x}\right) dx$$

$$= n\int_0^{\infty} x\phi(ax)\, dx + \frac{(n+1)n(n-1)}{3!}\int_0^{\infty} x^3\phi(ax)\, dx + \&c.$$

$$= \frac{n}{a^2}\int_0^{\infty} x\phi(x)\, dx + \frac{(n+1)n(n-1)}{a^4 . 3!}\int_0^{\infty} x^3\phi(x)\, dx + \&c.$$

Taking $b = a$, and replacing $a^2$ by $a$, we have

$$\int_0^{\infty} x^{2n-1}\phi\left(x - \frac{a}{x}\right) dx = na^{n-1}P_1 + \frac{(n+1)n(n-1)}{3!} a^{n-2}P_3 + \&c.;$$

while (1) treated in the same way gives

$$\int_0^{\infty} x^{2n} f\left(x - \frac{a}{x}\right) dx = a^n A_0 + \frac{(n+1)n}{2!} a^{n-1}A_2 + \&c.,$$

which is Boole's formula (*loc. cit.* p. 783).

§ 6. As an example of (2) I give the evaluation of the integral

$$\int_0^{\infty} x^{2n-1} \operatorname{cosech}\left(x - \frac{a}{x}\right) dx$$

(where $\operatorname{cosech} u$ denotes the hyperbolic cosecant of $u$).

[1] I may here note an erratum in Cauchy's third corollary (*loc. cit.* in § 1, p. 56), viz. in (17) and the left-hand side of (18) $\cos tx$ should be $\cos t\,(x - x^{-1})$.

Since

$$\int_0^\infty x^{n-1} e^{-ax} dx = \frac{\Gamma(n)}{a^n},$$

it follows, by addition, that

$$\frac{1}{\Gamma(n)} \int_0^\infty x^{n-1} (e^{-x} + e^{-3x} + e^{-5x} + \text{\&c.}) dx = \frac{1}{1^n} + \frac{1}{3^n} + \frac{1}{5^n} + \text{\&c.},$$

viz. that

$$\frac{1}{\Gamma(n)} \int_0^\infty \frac{x^{n-1} dx}{e^x - e^{-x}} = \frac{1}{2\Gamma(n)} \int_0^\infty x^{n-1} \operatorname{cosech} x \, dx = \left(1 - \frac{1}{2^n}\right) S_n,$$

where $$S_n = \frac{1}{1^n} + \frac{1}{2^n} + \frac{1}{3^n} + \frac{1}{4^n} + \text{\&c.}$$

Now $$S_{2n} = \frac{(2\pi)^{2n} B_n}{2\Gamma(2n+1)};$$

$B_n$ being the $n^{\text{th}}$ Bernoullian number, so that

$$\int_0^\infty x^{2n-1} \operatorname{cosech} x \, dx = 2\Gamma(2n)\left(1 - \frac{1}{2^{2n}}\right) \frac{(2\pi)^{2n} B_n}{2\Gamma(2n+1)}$$

$$= \frac{(2^{2n} - 1)\pi^{2n}}{2n} B_n,$$

and we thus find from (2) that

$$\int_0^\infty x^{2n-1} \operatorname{cosech}\left(x - \frac{a}{x}\right) dx$$

$$= \frac{n}{2!}(2^2-1)B_1\pi^2 a^{n-1} + \frac{n(n^2-1^2)}{4!}(2^4-1)B_2\pi^4 a^{n-2}$$

$$+ \frac{n(n^2-1^2)(n^2-2^2)}{6!}(2^6-1)B_3\pi^6 a^{n-3}$$

$$\ldots + (2^{2n-2}-1)\pi^{2n-2} B_{n-1} a + \frac{(2^{2n}-1)\pi^{2n} B_n}{2n},$$

the general term being

$$\frac{n(n^2-1^2)\ldots\{n^2-(r-1)^2\}}{(2r)!}(2^{2r}-1)\pi^{2r} B_r a^{n-r}.$$

§ 7. In this example the integral contains an infinite element corresponding to $x = \sqrt{a}$, and the value obtained is the principal value of the integral. This is generally true of all results derived from (2), as can be easily proved. It is interesting to notice that in transforming

$$\int_0^\infty \left(x^{2n} - \frac{1}{x^{2n}}\right) \phi \left(x - \frac{1}{x}\right) \frac{dx}{x}$$

into

$$2\int_0^\infty x^{2n-1} \phi \left(x - \frac{1}{x}\right) dx,$$

we suppose that the latter has its principal value for $x = 1$, if $\phi (x - x^{-1})$ should become infinite when $x = 1$: for writing the former integral

$$\left\{\int_0^{1-\lambda} + \int_{1+\lambda'}^\infty\right\} \left(x^{2n} - \frac{1}{x^{2n}}\right) \phi \left(x - \frac{1}{x}\right) \frac{dx}{x}$$

($\lambda$, $\lambda'$ infinitesimal), it becomes by transformation

$$\left\{\int_0^{1-\lambda} + \int_{1+\lambda}^\infty\right\} x^{2n-1} \phi \left(x - \frac{1}{x}\right) dx + \left\{\int_0^{1-\lambda'} + \int_{1+\lambda'}^\infty\right\} x^{2n-1} \phi \left(x - \frac{1}{x}\right) dx,$$

that is, the principal value of the integral is to be taken.

§ 8. As an example, in which the integral contains no infinite element, let

$$Q_{2n-1} = \int_0^\infty x^{2n-1} \left(x - \frac{a}{x}\right)^2 \operatorname{cosech} \left(x - \frac{a}{x}\right) dx,$$

then $$P_{2r-1} = \int_0^\infty x^{2r+1} \operatorname{cosech} x \, dx,$$

so that the value of $Q_{2n-1}$ is

$$\frac{n}{4} (2^4 - 1) B_2 \pi^4 a^{n-1} + \frac{n (n^2 - 1^2)}{1.2.3.6} (2^6 - 1) B_3 \pi^6 a^{n-2} \ldots$$

$$+ \frac{2n-2}{2n} (2^{2n} - 1) \pi^{2n} B_n a + \frac{(2^{2n+2} - 1) \pi^{2n+2} B_{n+1}}{2n+2}.$$

*Addition—added November* 7, 1876.

The following proof of the fundamental theorem that, $f$ being even,

$$\int_0^\infty f\left(x - \frac{1}{x}\right) dx = \int_0^\infty f(x) \, dx,$$

is due to Professor Cayley, and was communicated by him to the Meeting after the reading of this paper,

We have

$$\int_0^\infty f\left(x-\frac{1}{x}\right)dx = \int_0^1 f\left(x_1-\frac{1}{x_1}\right)dx_1 + \int_1^\infty f\left(x_2-\frac{1}{x_2}\right)dx_2.$$

Put $$x_1 - \frac{1}{x_1} = -y,\ x_2 - \frac{1}{x_2} = y,$$

and the integral

$$= -\int_0^\infty f(y)\,dx_1 + \int_0^\infty f(y)\,dx_2$$

$$= \int_0^\infty f(y)(dx_2 - dx_1) = \int_0^\infty f(y)\,dy,$$

for $$x_1 + x_2 - \frac{1}{x_1} - \frac{1}{x_2} = 0,$$

so that $$1 - x_1x_2 = 0,$$

whence $$x_2 - x_1 = y,\ \text{and}\ dx_2 - dx_1 = dy.$$

(2) Professor T. McK. Hughes, *On a Series of Specimens illustrating the Formation, Weathering, and Fracture of Flint: with Note by* Professor Stuart.

Professor Hughes exhibited three series of specimens in illustration of the mode of (1) formation, (2) weathering, and (3) fracture of flint, the first two being selected chiefly with a view to the last.

He produced proofs that the supposed faulted and re-cemented flints were generally only flint that had irregularly replaced jointed chalk, the formation of the flint being arrested by the joints. In the case of the banded flints he exhibited and distinguished two kinds—one in which infiltration had taken place all round the outside, often a good test of the drift origin of certain fragments; and the other in which a difference of texture, due generally to some included organism, had determined and limited the area over which infiltration had produced bands of colour. He pointed out that these differently coloured included portions, whether banded or not, affected the fracture, as they also depended upon the texture of the flint, but that the bands themselves had little or no influence upon the fracture.

He then drew attention to a series of specimens which shewed that when flint or other material of a similar texture was struck by any object such as a round-headed hammer, so that the blow was symmetrically distributed over a small area, a bruise was produced which on weathering flaked off all round a small cone having an angle at its apex of about 110°, and that when the whole had flaked away a small smooth basin was left. But if, and only if, the blow was sufficiently intense to break the flint up, this cone was found to truncate a larger cone whose apex had an angle of about 30°.

He pointed out that modifications of this double cone structure explained the "rings" and "bulb of percussion" which were appealed to as evidence of the direction of blows on which arguments were founded as to the origin of some stone implements.

Professor STUART pointed out that the explanation of the conical fracture shewn by Professor Hughes of the surface-pits was to be sought for in the homogeneous character of the substance. The primary cone bore a strong resemblance to that frequently seen in specimens of iron fractured by pressure; the cause of the formation of which was the shearing stress called into play by the pressure. According to the usual imperfect theory of elasticity the vertical angle of this cone would be 90°, but in iron thus fractured by pressure without lateral constraint it was less than that, owing apparently to the tangential action between contiguous layers of the nature of friction, *i. e.* increasing with the pressure. This action undoubtedly existed in other substances, and the vertical angle of the cone in any such fractures as those exhibited by Professor Hughes would thus depend on the direction of greatest pressure called into play by the blow, the circumstances being those of lateral restraint. Professor Stuart exhibited a drawing of a piece of oak which had been subject to great pressure, and which shewed by the condition of distortion of its fibres the planes of greatest shearing stress, inclined at an angle of about 110° in the permanently distorted piece of oak. He proceeded to shew that the texture of the primary cone would be in general destroyed, so that in process of time it would be weathered off, leaving the pits pointed out by Professor Hughes.—As to the secondary cone, he also distinguished the similarity which it bore to the form of a piece of iron punched out by a die resembling the head of the hammer by which the fracture in the flint in question was caused. —The irregular conchoidal fractures were due to the reflection of the wave produced by the impact from the various bounding surfaces of the specimen of flint.

*November* 20, 1876.

PROF. J. CLERK MAXWELL, F.R.S., PRESIDENT, IN THE CHAIR.

The following Communications were made to the Society by

(1) MR O. FISHER, M.A., *On the Effect of Convective Currents upon the Distribution of Heat in a Borehole.*

This paper was supplementary to one read by the Author on Nov. 29, 1875. The temperatures observed in the boring at Speremberg near Berlin, which attained a depth of upwards of 4000 feet, were reduced to a mean law by Professor Mohr of Bonn[1], and shewn by the Author to conform closely to those expressed by a parabolic curve, having its axis horizontal, and its vertex as a depth of 5171 feet, expressed by the equation

$$V = -\frac{251}{10^8}x^2 + 0\cdot012982x + 7\cdot1817,$$

$V$ being the temperature expressed in Reaumur's scale, and $x$ the depth.

Previous observations on underground temperature having led to the conclusion that the temperature curve within the earth's superficial strata is a straight line, expressing an increase of temperature varying as the increase of depth at the rate of 1° Fah. for about 50 to 60 feet of descent, the object of the present paper was to shew that the deviation of the curve from the rectilinear towards the parabolic form at Speremberg was due to convection currents.

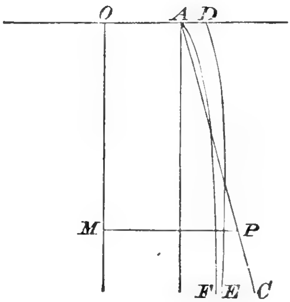
An elaborate account of the Observations taken in the Speremberg boring has been given by Dunker in a paper entitled "Ueber die Benutzung tiefer Bohrlöcher zur Ermittelung der Temperatur des Erdkörpers und die desshalb in dem Bohrloche I zu Sperenberg auf Steinsalz angestellten Beobachtungen." Von Herrn Dunker in Halle a. S.

Dunker's table shews that, when these currents were interrupted, the temperature of the water in the borehole fell in the upper part of the boring for a depth of somewhere between 100 and 300 feet, and below that depth rose, the difference between the temperatures taken when the currents were interrupted and when they were not so amounting sometimes to nearly 2° R. It

[1] See *Nature*, Oct. 21, 1875.

was argued that this agrees with the result that might be expected; and that the temperature curve corresponding to this effect would tend to a parabolic form.

For suppose $OA$ to represent the temperature at the surface; $AC$ the temperature curve within the body of the rock, the




ordinate increasing proportionally to the depth. If there were no convection currents, $AC$ would be likewise the curve of temperature of the water in the borehole. Next, suppose that there are convection currents; but that no heat is radiated away from the surface of the water: then the currents, warming the upper portion of the column of water and cooling the lower, will cause its curve of temperature to deviate from $AC$ in some such manner as does $DE$, intersecting $AC$. But at the surface of the water in a large borehole (in the present case a foot across) the temperature would be reduced nearly to that of the atmosphere by radiation and evaporation; so that the interval $AD$ would almost disappear, and the temperature curve be drawn towards $OM$, but more so in the upper than in the lower part. Thus it would assume a somewhat parabolic form, intersecting the temperature curve of the rock at no great distance from the surface.

Dunker seems to have taken the difference of temperature when the currents were interrupted and when they were not so, as indicating the difference of temperature between the water and the rock mass. But, in order to obtain the true rock temperature by interrupting the currents, it would have been necessary to have kept the thermometer down in the included water for a much longer period than was done, because when the rock temperature had, by long exposure to the action of the currents, been reduced below its true temperature for a considerable distance in laterally from the borehole, it would take a long while to recover itself. An instance was cited from the well at La Chapelle, Paris,

where it was shewn that five days were not sufficient to allow the heat, arising from the action of the boring tool, to be dissipated.

Although then the boring at Speremberg appears to have offered in some respects unusual advantages, both on account of its enormous depth and from the remarkable homogeneousness of the strata (283 feet being in gypsum and the remainder in pure rock-salt); nevertheless there are grounds for doubting whether the temperatures obtained at that place afford so good direct indications of the law of increase of temperature in the earth's crust, as those which have been made in smaller perforations, where the currents have been more opposed by the friction of the sides. At any rate there seems no reason to assent to Prof. Mohr's conclusion, that the observations in this boring prove that the cause of the heat of the crust of the earth must be sought within the crust itself.

(2) Prof. HUGHES, *On the Evidence for Pre-glacial Man.*

After having briefly reviewed some of the older evidence on which man had been referred to Pliocene, or even Miocene times, in all those cases in which he had an opportunity of examining the locality and circumstances of the discovery, he said that he had been satisfied that there was no foundation for the inference, and in the others he pointed out sources of error in the observations and various suspicious circumstances which seemed to him to warrant the rejection of the evidence as yet adduced.

He criticised more at length the evidence brought forward by Mr Tiddeman from the Victoria Cave—where, however, he allowed the best case had been made out, and where he thought more evidence might be obtained. He believed, however, that the boulder clay which overlapped the cave earth, in which a human fibula had been found, had fallen rather sideways over the pre-existing talus from a pipe of boulder-clay similar to many others which occur over the hill above the cave. The boulder-clay last found on the floor of rock at the mouth of the cave did not overlap the cave-earth and so need not be further referred to. The laminated clay he considered to be due to the sediment from ordinary flood-water after rains, and to be still in process of formation in similar caves. The clay in the talus on the inside of the boulder-clay he thought was only the mud which had been washed in between the stones when the water was ponded back in the cave by the bank of boulder-clay at the mouth.

In the cases adduced by Mr Skertchley from the loams of Norfolk and Suffolk he thought that not only was there no sufficient evidence to prove that remains of man had been found in glacial beds, but, on the contrary, that there was abundant evi-

dence to prove that the beds in which the flint implements occurred belonged to the valley gravels and brickearths of the same age as those from which they have long been known. By reference to sections he showed that there were at least three different horizons at which similar loams occurred in that district, omitting the most recent subaerial deposits, and that it was owing to a wrong identification of these loams that the mistake had arisen.

*December 4, 1876.*

PROF. CLERK MAXWELL, F.R.S., PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:

(1) MR J. W. L. GLAISHER, M.A., F.R.S.: *Preliminary account of the results of an enumeration of the primes in Dase's tables* (6,000,000 to 9,000,000).

The existing factor tables are: Chernac's *Cribrum Arithmeticum* (1811), giving all the divisors of every number up to 1,020,000; Burckhardt's *Tables des diviseurs* (1814—1817), giving the least divisor of every number up to 3,036,000; and Dase's *Factoren Tafeln* (1862—1865), giving the least divisor of every[1] number from 6,000,000 to 9,000,000. There is thus left a gap of three millions for which there are no printed tables; the tables exist in manuscript at Berlin, but have not been published.

In 1871 I commenced the enumeration of the primes in the six millions over which the published tables extend. The work was performed in duplicate by two computers, quite independently: the two enumerations were then read with one another and the discrepancies marked. All the doubtful numbers in both were then examined independently, and the two pieces of work were brought into agreement. Subsequently one of them was examined *de novo* throughout with the original tables.

A short account of the enumeration, as far as it had then proceeded, together with an abstract of the results for two of the millions, was published in the *Report of the British Association*, for 1872[2]. I have there given tables showing the agreement of the numbers of primes counted with the theoretical numbers derived from the logarithm-integral formula of Tchebycheff and Hargreave, for the second and ninth millions, arranged in groups

[1] Multiples of 2, 3 or 5 are excluded from Chernac's, Burckhardt's and Dase's tables, as for such numbers the least factor is determined either at sight or almost at sight.

[2] 'On the law of distribution of prime numbers.' *Transactions of the Sections*, pp. 19—21.

of 50,000: the second million was chosen for publication in preference to the first, chiefly because results derived from the counting of primes in the first million had been already published by Legendre, Hargreave, and others. Soon afterwards I became acquainted with the enumerations printed among the posthumous works of Gauss (Werke, t. III. pp. 436—447, 1863), and I found many discrepancies between these results and my own. This, taken in conjunction with the great difficulty of attaining not only accuracy, but the certainty of accuracy, in an enumeration which is of so troublesome a kind, led me to lay aside the work till 1874, when I had a considerable portion of it recomputed, and satisfied myself that the enumeration found among Gauss's papers contained many errors.

Several months ago I recommenced the whole again (beginning with the ninth million and proceeding downwards), the work being very carefully performed by a fresh computer, who had had no connexion with the previous enumerations. The ninth and eighth millions were examined with Dase's tables, and no errors were found; and from the care taken throughout I have a great degree of confidence that any error would have been detected: a portion of the work I did myself. There was some reason to think that the original enumeration for the seventh million was not entitled to so much credit as that for the eighth and ninth millions, and accordingly I had this million recomputed *de novo*, without reference to the existing enumerations. This new enumeration was performed with great care, and, on comparison with the old one, it was found to be absolutely free from error: there were two discrepancies, one of which was due to an uncertainty in the printed table of Dase, and the other to an error of a unit in the old calculation (the number had originally been right, and had been altered from right to wrong on the final examination).

The enumeration for the other three millions (1 to 3,000,000) is in progress; and I propose also to calculate, probably for groups of 10,000 or 50,000, the theoretical values given by the $\operatorname{li} x$ formula, and also by Legendre's formula $\dfrac{x}{\log x - 1{\cdot}08366}$; taking account also of Riemann's investigation, "Ueber die Anzahl der Primzahlen unter einer gegebenen Grösse" (Bernhard Riemann's *Gesammelte Mathematische Werke*, Leipzig, 1876, pp. 136—144).

The work has been so long on hand, and—however simple it may seem at first sight—is shown by experience to require the expenditure of so much care and time, in consequence of the extreme difficulty of attaining absolute accuracy, that, now that I am satisfied of the correctness of the enumeration as far as relates to the three millions over which Dase's tables extend, I am

desirous of communicating the general results to the society, without stopping for the calculation of the theoretical values, or attempting a discussion. These of course will be added hereafter when the whole enumeration is complete.

Tables I, II, III which accompany this paper give the results of the enumeration, the arrangement being the same as in Gauss's tables for the second and third millions, except that each table refers to 1,000,000 natural numbers instead of to 100,000.

The explanation of the tables is simple. If for convenience of expression we call the hundred numbers between $100n$ and $100(n+1)$, a 'century' (so that *e.g.* the hundred numbers between 6,000,000 and 6,000,100 form a century), then the table shows the number of centuries in each group of 100,000 which contain no prime, the number of centuries each of which contains one prime, the number of centuries each of which contains two primes, &c. Thus of the thousand centuries between 6,000,000 and 6,100,000 two centuries are composed wholly of composite numbers, two centuries contain each one prime, seventeen centuries contain each two primes, fifty-two contain each three primes, and so on. Of the thousand centuries between 6,100,000 and 6,200,000 there is no century consisting wholly of composite numbers, there is one century only that contains one prime, &c.

The numbers at the foot of each column give the total number of primes in the group of 100,000 to which the column has reference, thus between 6,000,000 and 6,100,000 there are 6,397 primes; between 6,100,000 and 6,200,000 there are 6,402 primes, &c.

It will be noticed that in the eighth million there are two centuries that contain 15 primes; but no century in either the seventh or ninth million contains so many. There are, however, three centuries in the seventh million, two in the eighth, and two in the ninth, each of which contains 14 primes.

It is interesting to note how slowly the numbers of primes in the successive millions diminish. The seventh million contains 63,799 primes, the eighth 63,158, and the ninth 62,760: so that there are only 641 primes less in the eighth than in the seventh million, and only 398 primes less in the ninth than in the eighth million.

The numbers of primes in each quarter million from 6,000,000 to 9,000,000 are:

| | seventh million. | eighth million. | ninth million. |
|---|---|---|---|
| First quarter | 15,967 | 15,851 | 15,712 |
| Second ,, | 15,941 | 15,772 | 15,652 |
| Third ,, | 15,950 | 15,768 | 15,746 |
| Fourth ,, | 15,941 | 15,767 | 15,650 |
| Total........... | 63,799 | 63,158 | 62,760 |

and the slowness with which the numbers diminish is apparent. The average number of primes throughout all the three millions is about 6·3 per cent.

In Dase's tables there is nothing to show whether the number 6,036,637 is or is not a prime. This is due to a fault in printing, the space being blank through the type having slipped back. In Table I, and in the numbers given above, I have assumed that 6,037,637 *is* a prime. This is, of course, merely a provisional assumption, as I shall hereafter decide the question by trial, but certainly the number has no very small divisor.

It is perhaps desirable to state, although it follows from what has been already said, that the numbers given in the 'primes counted' column of my British Association paper, for the ninth million, have all received confirmation by the recent examination of this million.

In regard to the work now in progress, I have thought it safer that the new enumeration should be quite distinct from the old, and accordingly it is being proceeded with entirely *de novo*, as if the old enumeration were not in existence. For the new enumeration two sets of blank forms were printed, the one to contain the number of primes in each century, there being places for 400 centuries (corresponding to 40,000 natural numbers) on each form, which is of the size of a somewhat large foolscap page, and the other to accommodate tables similar to Tables I, II, III. These are found to be a great convenience. In the old enumerations all the forms were ruled by hand, and the gain both in accuracy and clearness which results from the employment of the printed sheets is even greater than I expected it would be.

## TABLE I.

6,000,000 to 7,000,000.

| $n$ | Number of centuries each of which contains $n$ primes. | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 6,000,000 to 6,100,000 | 6,100,000 to 6,200,000 | 6,200,000 to 6,300,000 | 6,300,000 to 6,400,000 | 6,400,000 to 6,500,000 | 6,500,000 to 6,600,000 | 6,600,000 to 6,700,000 | 6,700,000 to 6,800,000 | 6,800,000 to 6,900,000 | 6,900,000 to 7,000,000 | 6,000,000 to 7,000,000 |
| 0 | 2 | 0 | 0 | 2 | 0 | 0 | 0 | 1 | 0 | 1 | 6 |
| 1 | 2 | 1 | 4 | 2 | 4 | 1 | 1 | 4 | 4 | 5 | 28 |
| 2 | 17 | 19 | 20 | 19 | 21 | 16 | 19 | 15 | 15 | 12 | 173 |
| 3 | 52 | 57 | 37 | 42 | 42 | 50 | 44 | 45 | 57 | 56 | 482 |
| 4 | 116 | 109 | 111 | 110 | 112 | 92 | 109 | 102 | 89 | 99 | 1049 |
| 5 | 140 | 147 | 163 | 169 | 174 | 157 | 168 | 147 | 169 | 169 | 1603 |
| 6 | 189 | 187 | 182 | 191 | 172 | 204 | 206 | 217 | 208 | 192 | 1948 |
| 7 | 204 | 192 | 190 | 187 | 195 | 208 | 182 | 197 | 177 | 184 | 1916 |
| 8 | 130 | 132 | 148 | 145 | 147 | 137 | 123 | 148 | 116 | 140 | 1366 |
| 9 | 79 | 96 | 75 | 84 | 79 | 88 | 100 | 68 | 90 | 81 | 840 |
| 10 | 43 | 38 | 44 | 33 | 28 | 27 | 33 | 39 | 49 | 40 | 374 |
| 11 | 18 | 13 | 22 | 9 | 20 | 14 | 13 | 11 | 22 | 14 | 156 |
| 12 | 5 | 6 | 4 | 5 | 5 | 5 | 2 | 3 | 4 | 7 | 46 |
| 13 | 2 | 2 | 0 | 2 | 1 | 0 | 0 | 3 | 0 | 0 | 10 |
| 14 | 1 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 3 |
| No. of primes | 6,397 | 6,402 | 6,425 | 6,337 | 6,347 | 6,402 | 6,338 | 6,375 | 6,411 | 6,365 | 63,799 |

The last line in each column shows the number of primes in the group to which the column has reference.

## TABLE II.

7,000,000 to 8,000,000.

| *n* | Number of centuries each of which contains *n* primes. | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 7,000,000 to 7,100,000 | 7,100,000 to 7,200,000 | 7,200,000 to 7,300,000 | 7,300,000 to 7,400,000 | 7,400,000 to 7,500,000 | 7,500,000 to 7,600,000 | 7,600,000 to 7,700,000 | 7,700,000 to 7,800,000 | 7,800,000 to 7,900,000 | 7,900,000 to 8,000,000 | 7,000,000 to 8,000,000 |
| 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 2 | 0 | 4 |
| 1 | 2 | 4 | 2 | 1 | 0 | 5 | 3 | 7 | 1 | 5 | 30 |
| 2 | 18 | 18 | 19 | 24 | 18 | 17 | 12 | 26 | 11 | 8 | 171 |
| 3 | 61 | 55 | 58 | 50 | 62 | 50 | 44 | 53 | 47 | 61 | 541 |
| 4 | 96 | 116 | 92 | 110 | 108 | 109 | 114 | 96 | 120 | 105 | 1,066 |
| 5 | 150 | 159 | 167 | 173 | 172 | 171 | 172 | 196 | 156 | 175 | 1,691 |
| 6 | 196 | 201 | 193 | 199 | 205 | 202 | 197 | 190 | 194 | 216 | 1,993 |
| 7 | 195 | 163 | 189 | 155 | 163 | 175 | 186 | 167 | 190 | 171 | 1,754 |
| 8 | 143 | 144 | 148 | 155 | 132 | 130 | 144 | 122 | 142 | 134 | 1,394 |
| 9 | 86 | 85 | 77 | 77 | 78 | 80 | 75 | 80 | 84 | 65 | 787 |
| 10 | 31 | 31 | 37 | 40 | 38 | 41 | 29 | 40 | 34 | 39 | 360 |
| 11 | 20 | 14 | 14 | 12 | 15 | 14 | 17 | 18 | 14 | 17 | 155 |
| 12 | 1 | 6 | 3 | 1 | 5 | 3 | 7 | 5 | 5 | 4 | 40 |
| 13 | 1 | 1 | 1 | 3 | 3 | 1 | 0 | 0 | 0 | 0 | 10 |
| 14 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 2 |
| 15 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 2 |
| No. of primes | 6,369 | 6,306 | 6,348 | 6,299 | 6,301 | 6,305 | 6,347 | 6,245 | 6,364 | 6,274 | 63,158 |

The last line in each column shows the number of primes in the group to which the column has reference.

# TABLE III.

8,000,000 to 9,000,000.

| $n$ | Number of centuries each of which contains $n$ primes. | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 8,000,000 to 8,100,000 | 8,100,000 to 8,200,000 | 8,200,000 to 8,300,000 | 8,300,000 to 8,400,000 | 8,400,000 to 8,500,000 | 8,500,000 to 8,600,000 | 8,600,000 to 8,700,000 | 8,700,000 to 8,800,000 | 8,800,000 to 8,900,000 | 8,900,000 to 9,000,000 | 8,000,000 to 9,000,000 |
| 0 | 1 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 1 | 4 |
| 1 | 1 | 3 | 5 | 3 | 4 | 3 | 2 | 3 | 6 | 4 | 34 |
| 2 | 15 | 20 | 16 | 18 | 19 | 21 | 15 | 19 | 22 | 13 | 178 |
| 3 | 61 | 55 | 52 | 59 | 63 | 43 | 59 | 63 | 62 | 53 | 570 |
| 4 | 111 | 96 | 120 | 88 | 93 | 120 | 108 | 109 | 116 | 117 | 1,078 |
| 5 | 189 | 176 | 162 | 197 | 182 | 172 | 185 | 143 | 159 | 177 | 1,742 |
| 6 | 195 | 212 | 198 | 191 | 201 | 182 | 190 | 197 | 204 | 196 | 1,966 |
| 7 | 166 | 168 | 189 | 202 | 177 | 188 | 167 | 189 | 167 | 175 | 1,788 |
| 8 | 130 | 136 | 124 | 98 | 128 | 126 | 141 | 139 | 126 | 130 | 1,278 |
| 9 | 70 | 76 | 72 | 74 | 75 | 86 | 79 | 92 | 82 | 72 | 778 |
| 10 | 37 | 41 | 41 | 44 | 39 | 38 | 33 | 36 | 34 | 47 | 390 |
| 11 | 16 | 8 | 16 | 21 | 18 | 12 | 15 | 8 | 18 | 11 | 143 |
| 12 | 5 | 6 | 3 | 2 | 0 | 7 | 6 | 1 | 4 | 4 | 38 |
| 13 | 3 | 3 | 1 | 1 | 0 | 2 | 0 | 1 | 0 | 0 | 11 |
| 14 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| No. of primes | 6,250 | 6,301 | 6,283 | 6,285 | 6,245 | 6,326 | 6,281 | 6,299 | 6,220 | 6,270 | 62,760 |

The last line in each column shows the number of primes in the group to which the column has reference.

(2) PROFESSOR F. W. NEWMAN : *A twelve place table of the exponential function.* Communicated by Mr J. W. L. Glaisher.

(*Abstract.*)

The table gives $e^{-x}$ from $x = 0{\cdot}000$ to $x = 5{\cdot}400$ at intervals of 0·001 to twelve places of decimals.

The mode of formation was as follows: the values of $e^{-x}$ for integral values of $x$ were first calculated to 16 decimals; this table ends of itself when $e^{-x}$ does not affect the 16th decimal, viz. when $x = 38$. Next the intervals were halved, so as to allow to $x$ the form $n + \frac{1}{2}$. The author then interpolated between $x = n$ and $x = n + \frac{1}{2}$, so as to give to $x$ the successive values $n$, $n + 0{\cdot}1$, $n + 0{\cdot}2$, $n + 0{\cdot}3$, $n + 0{\cdot}4$, $n + 0{\cdot}5$. Thus a second table was formed with $x$ increasing by 0·1 at each step from $x = 0$ till $e^{-x} = 0$; all to 16 decimals.

The author next calculated, with 12 decimals only, the intermediate intervals, so that $x$ might increase by 0·01 at each step. First the intervals were halved so as to give to $x$ the form $n + \frac{1}{10}n' + 0{\cdot}05$, $n'$ being a digit: and then four new values of $x$ between each successive pair of values were filled in by interpolation. A third table was thus completed, which ends at $x = 27{\cdot}63$. At this time it was not the intention of the author to proceed further, and though the work was performed to 13 decimals, the last figure was not retained.

Afterwards a fourth table was calculated, in which the increase was only 0·001 at each step: this extends from $x = 0$ to $x = 5{\cdot}400$, and forms the contents of the present communication to the society.

The whole work was performed by the author's own hand, and the only formula used at all in the calculations or interpolations was

$$e^{\pm h} = 1 \pm \frac{h}{1} + \frac{h^2}{1 \,.\, 2} \pm \frac{h^3}{1 \,.\, 2 \,.\, 3} + \&c.;$$

and since this is rigidly accurate the interpolations never involved small errors. It will be readily seen how by the subsidiary tables every fifth value in the great table was completely verified.

(3) Mr G. CHRYSTAL, B.A.: *On the effect of alternating induction currents on the galvanometer.*

Mr G. Chrystal and Mr Garnett exhibited before the Society some experiments on the Galvanometry of alternating induction currents. These experiments had been made by Mr Chrystal during an investigation into the accuracy of Ohm's Law, and will be found fully described in an article by him on Bi- and Unilateral Galvanometer deflection, in the *Philosophical Magazine* for December, 1876.

Mr Chrystal gave a sketch of his explanation of the phenomena, which he attributed to the influence of the alternating currents on the magnetism of the Galvanometer needle.

# PROCEEDINGS

OF THE

# Cambridge Philosophical Society.


*February* 12, 1877.

PROF. CLERK MAXWELL, PRESIDENT, IN THE CHAIR.

The following communication was made to the Society:

MR NEVILLE GOODMAN, M.A., *On a striking instance of Mimicry, with some notes on the phenomenon of Protective Resemblance.*

HAVING been for some years acquainted with a fine and well-known species of hornet (*Vespa orientalis*), and having taken and preserved several specimens from Sicily, Egypt, and Syria, in all which countries it is very abundant, I observed many of these hornets flying about in the neighbourhood of the Pools of Solomon on the W. side of Bethlehem, on the 29th May, 1876. These are so common in Egypt, where I had been spending the previous four months—the ruined temples of Upper Egypt being a favourite habitat—that I neglected them, and with them should have neglected the large (*Laphria*) fly, which so exactly resembles them that on the wing they are not distinguishable, if I had not chanced to look closely at one of these latter while in repose. I then captured several of these, and also several of the Vespæ in this same locality. After that I noted that both species were to be found in many other localities in Palestine.

Both the Vespa and the Laphria are highly predatory in their habits, and both are found abundantly in hollows sheltered from the wind and occupied by bushes and flowers where small flies abound. The Laphriæ seem confined to such localities, while the Vespæ, having a more general diet and needing to frequent places suited to their nidification and the collection of materials for that purpose, are seen in almost all kinds of places.

The points of resemblance are the following. They are for the most part confined to a similitude of colour, size, and shape; though the habits of the imitating species present some peculiarities which look like adaptations for the sake of resemblance.

In the Vespa, which is the imitated species, the clipeus and forehead above it is bright yellow. The eyes are dark brown, and

the nape russet brown. The whole thorax is russet brown (a yellow dot on the scale of the front wing being sometimes an exception to this).

Considering the abdomen of the female and worker to consist of six segments and that of the male of seven, the first segment is of the same russet brown colour as the thorax, but has a narrow (in some specimens a very narrow) border of yellow at the hind margin. The second segment is wholly russet brown. The third and fourth are bright yellow, with the exception of a spot or dash of russet brown on each side of each segment, with sometimes an indication of a dorsal line running through the light zone and ending in the front part of the third segment and the hind part of the fourth segment in a wedge-shaped mark. The fifth, sixth, (and in the male) seventh segments are russet brown.

The wings are rufous brown at the cortal margin, shading into dull brown at the inner margin. The legs are long, strong, and rufous throughout.

Corresponding to the colouring of this species the Laphria is of almost precisely the same russet brown colour, with the exception of the face and the fourth and fifth segments of the abdomen, which are bright yellow; while on each side of each of these segments is a dot of brown. These dots are minute but quite distinct, and sometimes there is an indication of a dorsal line. The wings are of russet brown somewhat richer than the hornet, while the hind and inner margins are washed with a duller brown. The limbs are stout and large, brown, and clothed with reddish hairs.

The only want of correspondence is in the hind margin of the first segment of the abdomen, in which the narrow band of yellow in the hornet has nothing corresponding to it in the fly.

I need hardly remark that the structures which underlie this apparent correspondence are wholly dissimilar, there being no closer homology between the hymenopterous and dipterous insect than between a zebra and a tiger. More than this, it is singular to find that the same visual effect is caused by wholly different circumstances; for, while it is the surface of the comparatively smooth armour of the hornet which is itself coloured russet brown and bright yellow, the colour of the Laphria is caused by a dense pile of ochreous hairs almost hiding the nearly black chitonic integument, and the yellow is wholly reflected from the hairs themselves.

Although the flight of the Laphria is, as one would suppose from the less proportional size of its wings, less sustained than that of the Vespa, while it lasts it is very much like it, and is accompanied by a buzzing sound, especially in starting and alighting.

The abdomen of the Vespa is conical and boldly arched in

profile, and its section circular; that of the Laphria is depressed, but, to favour the illusion, it always arches up its abdomen both longitudinally, doubling its tail beneath it, and transversely, so that its section is a meniscus. Mr Albert Goodman, who executed pictures of them for me, has illustrated this point. When the fly is seen either from above or obliquely either fore or aft, the illusion is complete, but in direct profile and from underneath the hollow deception is apparent. The gay deceiver is detected. I lay some stress on this device, because when the habit as well as the aspect of an insect promotes resemblance we have an independent witness to the teleological bearing of the imitation, and the argument presents itself to the mind with overwhelming force. Thus, to take a parallel from the British fauna, when the larvæ of the larger geometræ show such a striking resemblance to a jointed twig that they are called by the vulgar stick-caterpillars, and we further find that they are in the habit of standing immoveable with their four false legs grasping the stem of a bough, with their heads and remainder of their bodies lifted freely in the air at an angle corresponding to the spraying of the plant on which they feed; then the combined testimony of aspect and habit prove the utilitarian nature of the likeness.

In discussing the phenomena of mimicry Mr R. A. Wallace in his book on *Natural Selection* (pp. 76, 77) lays down three laws which govern it, and which I take the liberty of expressing in my own words.

In all cases of true mimicry

1. The imitated and imitating species are to be found in the same country, situation, and at the same season.

2. The imitated species (or group of species) are abundant in individuals, have a wide range, and are ascertained to have some special protection.

3. The imitating species have a less number of individuals and a more restricted range than the imitated.

These laws are connected by the theory, that the imitating species, by virtue of its resemblance, shares the advantages of the special protection of the imitated species by not suffering from the attacks of enemies who have acquired or possess the instinct to avoid the imitated species.

This instance conforms well to these laws,

1. In identity of position, since I caught both repeatedly in the same place at the same hour.

2. The imitated species is a dominant species. It is found all round the shores of the Mediterranean, and extends through Upper Egypt, Syria, and Arabia into Hindostan, and is one of the most common of insects in those countries. It has a special protection, since it is one of the largest and most formidable of the

aculeate hymenoptera, with a long sting, poison glands and bag. It lives in powerful communities, and has a very hard resisting armour. I have no means of judging of the comparative effectiveness of the sting of this species and that of our hornet, except from the exclamation of my Egyptian Reis, who, observing me catching them from the sint trees, began to pick them off with his naked fingers. I did not enquire the subsequent effect of the sting. The interests of science now make me more regret that I did not then make an enquiry which the interests of humanity should have suggested.

3. Turning to the imitated species we find the Fly, while it has a proboscis which is a very effective weapon to thrust into the soft bodies of small flies when held by its powerful limbs, yet it may be held in the fingers with impunity, and neither attempts nor can effect any wound with this weapon. The abdomen, too, is soft and flexible, quite unlike the hard resisting body of the hornet. In fact, this insect is quite without either offensive or defensive armour for lawful warfare. He is an assassin who carries a dagger wherewith he can do grievous mischief to his weak and unprotected victims, but is quite unfit for combat; while the hornet is an armed warrior, disciplined, lance-bearing, and mail-clad. As far as my experience goes the range of the Laphria is not only within, but vastly within, the limits of the range of the hornet. After some search I found three specimens in the crypts of the British Museum; they were unnamed and labelled as from Syria.

Altogether this is one of the most striking instances I have met with. Of course I am aware that instances of mimicry abound everywhere. We need not go to the Indian mountains or Brazilian forests or even to Syria to find them. They are to be detected as readily in our British fauna as in distant lands. It is perhaps therefore necessary to apologize for bringing a single instance of so common a phenomenon under your notice. I have done so for this reason. Though instances of mimicry are numerous they are in most instances masked, interfered with, and modified by antagonistic tendencies which render them questionable. We need therefore a few indisputable instances to establish the theory. We then should inquire into the causes or conditions of the strictness of the similitude in these few instances, and so obtain the key to the more complicated cases. Did time permit I think I could show from *a priori* considerations that the similitude in this case was likely to be close. Also that in other cases derived from our own fauna—cases which I am now collecting—that though they are equally genuine they are not likely to be so close, though they are more effective on account of the modification of verisimilitude. But the whole question is so complicated and

interesting that I must beg your attention to this branch of it at some future time.

The word 'mimicry' as applied to these now well-known facts is open to objection, inasmuch as it seems to express an *active* imitation, whereas the species accused of mimicry are of course passive. On the other hand, the term is precise as expressing a superficial as distinguished from a fundamental similitude. "Protective resemblance" is a better term in some respects, but this embraces a far wider group of facts. "Protective resemblance" might be applied, and is applied, to the resemblance of live animals to stones, sticks, rotten wood, bark, lichens, dead or rolled leaves, the dung of animals, &c., whereas "mimicry" can be only applied to the assimilation of one living being to another. It seems then desirable to retain the word "mimicry," since it can hardly be said to mislead, in order to define a branch of so wide a subject.

The phenomena of mimicry thus limited and characterized by laws are thought to be at once a subtil and a definite proof of the theory of the transmutation of species by natural selection. All, however, that can be fairly said is, that these phenomena are readily explained by that theory, and thus add to that vast cumulative proof which is the only one of which the theory is capable. Those who advocate the doctrine of fixity of species, and living in a universe where forces infringing on matter operate in such complicated combinations and antagonisms that the most stable and inert substances are liable to change and are reconstructed, yet believe that the most differentiated and complex organisms, composed of the most unstable and complex molecules, are the only bodies characterized by immobility, (these) can with facility add to the sum of their numerous unsupported hypotheses the conjecture that both the imitator and the imitated were not only so constituted from the beginning, but are held in their present relationship as to the relative number of their individuals and also as to their several enemies, by which alone the phenomena of mimicry can be teleological or rational.

All, however, that is required for the reception of any theory is that it shall explain more phenomena than any other theory, and be not absolutely inconsistent with any proven fact. These facts of mimicry appear to contribute to the mass of facts which this theory explains, but I cannot think that they afford definite proof of the theory.

Perhaps I may be allowed to refer to another connected resemblance. Long before these curious similitudes had received the explanation which I believe Mr Bates first originated, a nearly allied form of this dipterous insect resident in England received the name Asilus crabroniformis, from its likeness to our hornet.

A comparison of these two species with the other two will reveal at once that the similitude is in this instance much less masked. The Asilus crabroniformis is in bulk and width much inferior to our native hornet. Although the fourth, fifth, sixth and seventh segments of the abdomen are yellow, and so correspond with the last segment of the abdomen of the hornet, they have no maculation, as those of the hornet have. The velvety black of the three basal segments of the abdomen in the fly detract from the likeness also. Indeed, the dried and pinned-out insects are so unlike that one wonders that they could have been thought to be alike, and yet I can assure those who have not had the opportunity of observing them in the field, that when thus seen this fly cannot fail to remind one of the Vespa crabro, after which it is named.

Here then we have two similitudes of different degrees, to which many more might be taken in pairs from the same two families, and these would represent many gradations.

Now the gradation of an advantageous useful character, from a case in which the protective similitude can scarcely be doubted by any one to those in which it is barely recognizable, certainly seems to indicate a transmutability of species. If fixity were the law, we might expect perfection in a protective resemblance so far as is consistent with the habits and functions of the two species. Whereas, if all species be transmutable it will need long continuance of unchanged conditions to allow two utterly different species to approximate one another even in aspect, and there will be various stages in their approximation. In other words, where progression and evolution is the law, imperfection is incidental to perfection. If fixity be the law, anything short of absolute perfection is an unexplained fault in nature.

## *February* 26, 1877.

PROF. CLERK MAXWELL, PRESIDENT, IN THE CHAIR.

The following communication was made to the Society:

MR C. CREIGHTON, *On the order in which the secreting and the conducting parts of an acinous gland appear, in the individual development, and in the succession of animals.*

THE early appearance of the ducts in the embryonic development of acinous glands is the fact in their development that has

chiefly been made use of to frame a theory of their mode of origin. Notwithstanding the secondary and purely mechanical office that the ducts of a gland perform in the process of secretion, it is to those parts of the gland that, in the embryonic development, the largest germinal powers have been assigned. The exaggerated importance that has been attributed to the ducts in the history of a gland's development depends, no doubt, on the fact that the ducts of an acinous gland are the first parts of it that make their appearance. In the embryo the first indication of the future gland is a number of ducts extending, in a branching system, from a point on the surface into the tissue beneath. In the generally received explanation of the growth of the gland, the whole secreting structure is taken as growing out, by a process of budding, from the blind ends of the branching ducts. A germinal property is attributed to such linear tracts of cells; their terminal expansions are to be endowed with the property of secreting, although they themselves will have the merely mechanical property of conducting. In accordance with this view of the embryonic development the general plan of structure of a gland is taken to be that of a complex reduplication of the plane surface—skin or mucous membrane—upon which the secretion is discharged.

But the early appearance of the ducts in the embryonic development, which has thus determined the theory of gland-formation, is a reversal of the order in which the parts of a gland respectively appear in the evolution of the more complex glands from the simpler forms. The simplest glands may be said to be masses of secreting cells, with a merely rudimentary and casual provision for discharging the secretion; and the progress of the gland towards a higher type consists in the acquisition of a more permanent and more convenient system of conduits, or in a more rigid division between its secreting and its conducting parts. It is the object of this communication to show that the plan of structure of a complex gland is best explained by tracing the gland back, not to its embryonic form in the individual, but to its primitive condition in the series of animals; and further, to show that the inverted order in which its parts respectively occur in the embryo of the higher animal is a fact capable of explanation according to biological principles.

There are several familiar examples of glands that are of a complex type in the higher animals, but of a simpler arrangement in the lower. The simpler type is, generally speaking, the follicular. The salivary glands are follicular in insects, and acinous in higher animals. The lachrymal glands are follicular in Chelonians, but acinous in other vertebrates. J. Müller, to whom the last-mentioned fact is due, states also that he "has demonstrated the transition of the pancreatic cœca of fishes through a

series of intermediate steps to the cellular pancreas." The best example, for the present purpose, is the mammary gland. In its simplest form, in the *Monotremata,* it consists of 100—200 cœca or follicles, which converge to a small spot of the skin on each side of the abdomen, each follicle being attached to the skin by a separate neck. This is an example of a gland that is follicular at the beginning of the mammalian series, but is of a complex acinous type in most mammals. The steps by which the follicular type is changed into the acinous consist in certain acts of centralisation, by which single passages occupying the middle of the follicles are united, near their termination in the skin, to form one or more common outlets; while smaller and still smaller groups of follicles combine, at their outlying parts, to form their canals into ducts of the secondary and tertiary order. The centralising process may be represented diagrammatically in more than one way; the result is that the multitude of independent and co-ordinate outlets are replaced by a main system of conduits and subordinate branches. The starting point of the system of ducts is the passage that forms in the middle of each follicle, which is otherwise a mass of secreting cells. In the liver of *Carcinus moenas* Goodsir describes the rudimentary duct as an irregular passage existing in the midst of the cells near the attached end of the follicle. Such passages, according to the same authority, are formed by the breaking down of the ripest cells to yield the secretion. The irregular passage thus formed is, in the further adaptation, retained as a permanent outlet. Still further, the central channels of the several follicles join to form a common system of ducts, as already explained. At the same time the follicles, which make up the primitive gland, lose their individuality, and the organ passes into the acinous type.

The adaptation of the parenchyma, along certain central lines, to become a permanent system of ducts, is an economical process. The more permanent and convenient the outlets of the secretion are, the smaller is the bulk of secreting parenchyma required. The size of the mamma, during its active state, in *Ornithorhynchus* and *Echidna,* is relatively very much greater than in other mammals.

An explanation has now to be given of the fact that the branching system of ducts, which is the latest acquisition of the secreting gland, is the first part to appear in the embryo. The fact itself may be readily observed in the developing mamma of the guinea-pig, where a complete racemose system of ducts is found to extend over the whole area of the gland before any of the secreting acini have taken shape. The same fact may be readily made out in the development of the salivary glands. That reversal of the phylogenetic order may be accounted for by keeping in view the

different functional importance of the acini and the ducts respectively. Ducts are always mechanical in purpose, and constant in structure; but the secreting parenchyma of a gland is subject to constant cellular changes, more or less obvious. In the periodical changes of the mammary gland the constancy of the ducts as contrasted with the variability of the acini is readily noticed. According to a generalisation of Mr Herbert Spencer's (*Principles of Biology*, Vol. I. 371) there are two modes of development, the direct and the indirect. The indirect mode of development is for the embryo what the cumbrous process of adaptation has been in the progenitors; and all embryonic development is therefore at the outset indirect. But under certain circumstances the natural indirectness of the development tends to be supplanted by a more direct mode of formation. The embryo, or part of the embryo, "grows to its appointed shape by the shortest route." Certain classes of animals are distinguished by the directness of their development, and certain organs and parts of the body are in like manner distinguished. According to Mr Spencer, directness of development comes in in the embryo, where in the progenitor there has been constancy of conditions. If the two kinds of structure in the breast are compared in this respect, the ducts are those parts of the organ that are constant in their structure; if the explanation that has been given of the origin of ducts be correct, their constancy and permanency is their *raison d'être*. But the acini of a gland, such as the mammary gland, are constantly varying; their secreting activity depends for its existence on incessant changes of their cellular elements. If, then, those remarkable differences in the mature animal are reflected in the embryonic development, the difference would be that the ducts have a direct development, or, in Mr Spencer's words, that they "grow to their appointed shape by the shortest route." It is the directness of their development that has to be associated with the fact of their early appearance; but the explanation will lose its force if the complex system of ducts is taken as growing out from a central point by a process of budding. In the mamma of the guinea-pig there is reason to think that the ducts are laid down throughout its rudiment according to a pre-determined plan, and that they are formed by linear aggregations of the embryonic cells. The same mode of formation is referred to by Goodsir when he says, of glands in general, that "in certain instances it has been observed that the smaller branches of the duct are not formed by continued protrusion of the original blind sac, but are hollowed out independently in the substance of the blastema, and subsequently communicate with the ducts" (*Anatom. Memoirs*, Vol. II. 425).

Limiting the attention to the mammary gland, of whose

development in the guinea-pig I have published an account (*Journal of Anatomy and Physiology*, Vol. XI.), the early appearance of the ducts is not only no evidence that they are the germinal tracts of the whole secreting structure, but it is, on the other hand, a circumstance that might be expected when their purely mechanical purpose and permanency of structure in the adult is taken into account. Further, there has been no attempt on the part of writers on development to demonstrate the actual outgrowth of the acini from the blind ends of the ducts. The branching system of ducts has been seen extending at an early period over the area of the gland, and the unwarranted step has been taken of assuming the still farther expansion or budding of the smallest ducts to form the acini. In the paper above quoted I have described the formation of the mammary acini as a process of interstitial growth, whereby round spaces filled with secretory cells are developed from the matrix tissue of the gland at many scattered points, and at a time when the ducts are already there. The developing acini therefore find themselves ranged along the sides of ducts into which they burst, through the internal pressure of their developing cells. The same conclusion as to the development of the terminal secreting crypts of glands in general is stated in a summary way by Goodsir (l. c.).

It thus appears that, in the only cases where the development of the secreting structure has been specially looked for, it does not support the theory of the germinal property of the ducts. That remarkable endowment has been attributed to them no doubt on account of their early appearance. In the case of the mammary gland, the early appearance of the ducts has led to an erroneous theory of its growth; and it becomes an interesting question whether, in the case of other glands, the early appearance of the duct, or ducts, implies, as has been always hitherto assumed, that they are the germinal starting-points of the whole organ.

*March* 12, 1877.

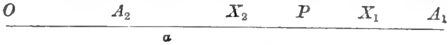
PROF. CLERK MAXWELL, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:

(1) Prof. J. CLERK MAXWELL. *On a Paradox in the Theory of Attraction.*

LET $A_1A_2$ be a straight line, $P$ a point in the same, $X_1$, $X_2$ corresponding points in the segments $PA_1$, $PA_2$.

Let the distances of these points from the origin $O$ measured




in the positive direction be $a_1$, $a_2$, $p$, $x_1$, $x_2$, respectively, and let the equation of correspondence between $x_1$ and $x_2$ be

$$\frac{1}{x_1-p}-\frac{1}{a_1-p}=\frac{1}{p-x_2}-\frac{1}{p-a_2} \quad \text{..............(1).}$$

If $x_1$ and $x_2$ vary simultaneously,

$$\frac{dx_1}{(x_1-p)^2}=-\frac{dx_2}{(p-x_2)^2} \quad \text{........................(2).}$$

Hence $x_1$ and $x_2$ move in opposite directions, and the lengths of the corresponding elements $dx_1$ and $dx_2$ (considered both positive) are as the squares of their respective distances from the point $P$.

If therefore $AB$ is a uniform rod of matter attracting inversely as the square of the distance, the attractions of the corresponding elements on a particle at the point $P$ will be equal and opposite.

Now by giving values to $x_1$, varying continuously from $a_1$ to $p$, we may obtain a corresponding series of values of $x_2$, varying from $a_2$ to $p$, and since every corresponding pair of elements $dx_1$ and $dx_2$ exert equal and opposite attractions on a particle at $P$, we might conclude that the attraction of the whole segment $A_1P$ on a particle at $P$ is equal and opposite to that of the segment $A_2P$ on the same particle.

But it is still more evident that if $A_2P$ is the greater of the two segments, and if we cut off $P\alpha = PA_1$ the attractions of $P\alpha$ and $PA_1$ on the particle at $P$ will be equal and opposite. But the attraction of $PA_2$ exceeds that of $P\alpha$ by the attraction of the part $\alpha A_2$, therefore the attraction of $PA_2$ exceeds that of $PA_1$ by a finite quantity, contrary to our first conclusion.

Hence our first conclusion is wrong, and for this reason. The attractions of any two corresponding segments $A_1X_1$ and $A_2X_2$ are exactly equal, but however near the corresponding points $X_1$ and $X_2$ approach to $P$, the attraction of each of the parts $X_1P$ and $X_2P$ on $P$ is infinite, but that of $X_2P$ exceeds that of $X_1P$ by a constant quantity, equal to the attraction of $A_2\alpha$ on $P$.

This method of corresponding elements leads to a very simple investigation of the distribution on straight lines, circular and elliptic disks and solid spheres and ellipsoids of fluids repelling according to any power of the distance.

The problem has been already solved by Green* in a far more general manner, but at the same time by a far more intricate method.

We have, as before, for corresponding values of $x_1$ and $x_2$

$$\frac{1}{x_1-p}-\frac{1}{a_1-p}=\frac{1}{p-x_2}-\frac{1}{p-a_1}\text{.................}(1).$$

Transposing

$$\frac{1}{x_1-p}+\frac{1}{p-a_1}=\frac{1}{p-x_2}+\frac{1}{a_1-p}.$$

Multiplying

$$\frac{(x_1-a_1)(x_1-a_2)}{(x_1-p)^2(a_1-p)(p-a_2)}=\frac{(a_2-x_2)(a_1-x_2)}{(p-x_2)^2(a_1-p)(p-a_2)}\text{...}(3).$$

If we write $$(a_1-x_1)(x_1-a_2)=y_1\text{ ....................}(4),$$

$$(a_1-x_2)(x_2-a_2)=y_2\text{ ....................}(5),$$

we find from equation (3)

$$\frac{x_1-p}{y_1}=\frac{p-x_2}{y_2}\text{ .......................}(6).$$

Let $\rho_1$, $\rho_2$ be the densities and $s_1$, $s_2$ the sections of the rod at the corresponding points $X_1$ and $X_2$, and let the repulsion of the matter of the rod vary inversely as the $n^{\text{th}}$ power of the distance, then the condition of equilibrium of a particle at $P$ under the action of the elements $dx_1$ and $-dx_2$ is

$$\rho_1 s_1 dx_1(x_1-p)^{-n}=-\rho_2 s_2 dx_2(p-x_2)^{-n}\text{...........}(7).$$

Eliminating $dx_1$ and $dx_2$ by means of equation (2), we find

$$\rho_1 s_1(x_1-p)^{2-n}=\rho_2 s_2(p-x_2)^{2-n}\text{ .................}(8),$$

and from this by means of equation (6) we obtain

$$\rho_1 s_1 y_1^{2-n}=\rho_2 s_2 y_2^{2-n}\text{.......................}(9),$$

as the condition of equilibrium between the elements.

The condition of equilibrium is therefore satisfied for every pair of elements by making

$$\rho s y^{2-n}=\text{constant}=C\text{ .................}(10).$$

* George Green. Mathematical Investigations concerning the laws of the equilibrium of fluids analogous to the electric fluid, with other similar researches. *Transactions of the Cambridge Philosophical Society*, 1833. (Read Nov. 12, 1832.) Ferrers' Edition of Green's Papers, p. 119.

In a uniform rod $s$ is constant, so that the distribution of density is given by the equation

$$\rho = Cy^{n-2} \quad\text{.........................(11).}$$

If $n = 2$, as in the case of electricity, the density is uniform.

We have already shown that when the density is uniform a particle not at the middle of the rod cannot be in equilibrium, but on the other hand any finite deviation from uniformity of density would be inconsistent with equilibrium. We may therefore assert that the distribution of the fluid when in equilibrium is not absolutely uniform, but is least at the middle of the rod, while at the same time the deviation from uniformity is less than any assignable quantity.

If the force is independent of the distance, $n = 0$ and

$$\rho = Cy^{-2} \quad\text{.........................(12),}$$

or if $r$ is the distance from the middle of the rod, $2l$ being the length of the rod,

$$\rho = \frac{C}{l^2 - r^2} \quad\text{.........................(13).}$$

If $C$ were finite, the whole mass would be infinite. Hence if the mass of fluid in the rod is finite it must be concentrated into two equal masses and placed at the two ends of the rod.

Let us next consider a disk on which two chords are drawn intersecting at the point $P$ at a small angle $\theta$, and let corresponding elements be taken of the two sectors so formed.

In this case the section of either sector is proportional to the distance of the element from the point of intersection, and therefore the two sections are proportional to the values of $y$ at the two elements. Hence if $\rho y^{3-n}$ is constant, the particle at the point of intersection will be in equilibrium.

If the edge of the disk is the ellipse whose equation is

$$1 - \frac{\xi^2}{a^2} - \frac{\eta^2}{b^2} = 0 \quad\text{.......................(14),}$$

and if at any point within it

$$1 - \frac{\xi^2}{a^2} - \frac{\eta^2}{b^2} = p^2 \quad\text{.......................(15),}$$

and if the length of a diameter parallel to the given chord is $2d$, then the value of $y$ for any point of the chord is

$$y = pd \quad \text{.........................(16).}$$

Hence if
$$\rho = Cp^{n-3} \quad \text{.........................(17),}$$

a particle placed at any point of the disk will be in equilibrium under the action of any pair of sectors formed by chords intersecting at that point, and therefore it will be absolutely in equilibrium.

When as in the case of electricity, $n = 2$,

$$\rho = Cp^{-1} \quad \text{.........................(18),}$$

the known law of distribution of density.

If the repulsion were inversely as the distance, the fluid would be accumulated in the circumference of the disk, leaving the rest entirely empty.

If the force were inversely as the cube of the distance, the density would be uniform over the surface of the disk.

Lastly, let us consider a solid ellipsoid, the equation of the surface being

$$1 - \frac{\xi^2}{a^2} - \frac{\eta^2}{b^2} - \frac{\zeta^2}{c^2} = 0,$$

and at any point within it let

$$1 - \frac{\xi^2}{a^2} - \frac{\eta^2}{b^2} - \frac{\zeta^2}{c^2} = p^2.$$

At any point of a chord drawn parallel to a diameter whose length is $2d$ the value of $y$ is $pd$.

If we consider a double cone of small angular aperture whose vertex is at a given point, and whose axis is this chord, the sections at two corresponding elements are in the ratio of the squares of the distances of the elements from the given point, and therefore in the ratio of the values of $p^2$ at these elements. Hence the condition to be satisfied is

$$\rho p^{4-n} = C_1 \text{ a constant.}$$

If this condition be fulfilled the fluid will be in equilibrium at every point of the ellipsoid.

If $n = 2$, $\rho = Cp^{-2}$

is the condition of equilibrium. But if $C$ is finite the whole mass of the fluid in the ellipsoid if distributed according to this law of

density would be infinite. Hence if the whole quantity of fluid is finite it must be accumulated entirely on the surface, and the interior will be entirely empty, as we know already.

If the force is inversely as the fourth power of the distance the density within the ellipsoid will be uniform.

(2) Prof. J. Clerk Maxwell. *On Approximate Multiple Integration between Limits by Summation.*

It is often desirable to obtain the approximate value of an integral taken between limits in cases in which, though we can ascertain the value of the quantity to be integrated for any given values of the variables, we are not able to express the integral as a mathematical function of the variables.

A method of deducing the result of a single integration between limits from the values of the quantity corresponding to a series of equidistant values of the independent variable was invented by Cotes in 1707, and given in his Lectures in 1709. Newton's tract *Methodus Differentialis* (see Horsley's edition of Newton's Works (1779) Vol. I. p. 521) was published in 1711.

Cotes' rules are given in his *Opera Miscellanea*, edited by Dr Robert Smith, and placed at the end of his *Harmonia Mensurarum.* He gives the proper multipliers for the ordinates up to eleven ordinates, but he gives no details of the method by which he ascertained the values of these multipliers.

Gauss, in his *Methodus nova Integralium Valores per Approximationem Inveniendi* (Göttingische gelehrte Anzeigen, 1814, Sept. 26, or Werke, III. 202) shows how to calculate Cotes' multipliers, and goes on to investigate the case in which the values of the independent variable are not supposed to be equidistant, but are chosen so as with a given number of values to obtain the highest degree of approximation.

He finds that by a proper choice of the values of the variable the value of the integral may be calculated to the same degree of approximation as would be obtained by means of double the number of equidistant values.

The equation, the roots of which give the proper values of the variable, is identical in form with that which gives the zero values of a zonal spherical harmonic.

*Double Integration.*

There is a particular kind of double integration which can be treated in a somewhat similar manner, namely, when the quantity

to be integrated is a function of a linear function of the two independent variables.

Thus if

$$I = \int_{x_1}^{x_2}\int_{y_1}^{y_2} u\,dx\,dy \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1),$$

where $u$ is a function of $r$, and

$$r = a + bx + cy \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2),$$

let

$$x = \frac{1}{2}(x_2 + x_1) + \frac{1}{2}p\,(x_2 - x_1) \quad\ldots\ldots\ldots\ldots(3),$$

$$y = \frac{1}{2}(y_2 + y_1) + \frac{1}{2}q\,(y_2 - y_1) \quad\ldots\ldots\ldots\ldots(4),$$

$$I = \frac{1}{4}(x_2 - x_1)(y_2 - y_1)\int_{-1}^{1}\int_{-1}^{1} u\,dp\,dq \quad\ldots\ldots\ldots\ldots(5).$$

If we write

$$r_0 = a + \frac{1}{2}b\,(x_2 + x_1) + \frac{1}{2}c\,(y_2 + y_1) \quad\ldots\ldots\ldots\ldots(6),$$

$$\beta = \frac{1}{2}b\,(x_2 - x_1)\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(7),$$

$$\gamma = \frac{1}{2}\gamma\,(y_2 - y_1)\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(8),$$

$$\zeta = \beta p + \gamma q \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(9),$$

we may consider $u$ as a function of $\zeta$ of the form

$$u = A_0 + A_1\zeta + A_2\zeta^2 + \&c.\ldots\ldots\ldots\ldots\ldots\ldots(10),$$

$$\frac{1}{4}\int_{-1}^{1}\int_{-1}^{1} u\,dp\,dq = A_0 + \frac{1}{3}A_2(\beta^2 + \gamma^2) + A_4\left(\frac{1}{5}\beta^4 + \frac{1}{9}\beta^2\gamma^2 + \frac{1}{5}\gamma^4\right)$$
$$+ \&c. \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(11).$$

Now let $u_0$ be the value of $u$ corresponding to $\zeta = 0$,

$u_1$ and $u'_1$ $\qquad\qquad \zeta = \pm\,\zeta_1$,

$u_2$ and $u'_2$ $\qquad\qquad \zeta = \pm\,\zeta_2$,

and if we assume

$$I = (x_2 - x_1)(y_2 - y_1)\,\{R_0u_0 + R_1(u_1 + u'_1) + R_2(u_2 + u'_2)$$
$$+ \&c.\} \ldots\ldots\ldots\ldots\ldots\ldots(12),$$

$$I = (x_2 - x_1)(y_2 - y_1)\,\{(R_0 + 2R_1 + 2R_2 + \&c.)\,A_0$$
$$+ (2R_1\zeta_1^2 + 2R_2\zeta_2^2 + \&c.)\,A_2 + \&c.\}\ldots\ldots(13),$$

then since the form of the function $u$, and therefore the values of the coefficients $A_0$, $A_1$, $A_2$, &c. must be considered entirely arbitrary, we may equate the coefficients of $A_0$, &c. in equations (11) and (13) as follows:

$$R_0 + 2R_1 + 2R_2 + \&c. \quad = 1 \qquad = B_0,$$

$$2R_1\zeta_1^2 + 2R_2\zeta_2^2 + \&c. = \frac{1}{3}\beta^2 + \frac{1}{3}\gamma^2 \qquad = B_1,$$

$$2R_1\zeta_1^4 + 2R_2\zeta_2^4 + \&c. = \frac{1}{5}\beta^4 + \frac{1}{9}\beta^2\gamma^2 + \frac{1}{5}\gamma^4 \qquad = B_2,$$

$$2R_1\zeta_1^6 + 2R_2\zeta_2^4 + \&c. = \frac{1}{7}\beta^6 + \beta^4\gamma^2 + \beta^2\gamma^4 + \frac{1}{7}\gamma^6 \qquad = B_3,$$

$$2R_1\zeta_1^8 + 2R_2\zeta_2^8 + \&c. = \frac{1}{9}\beta^8 + \frac{4}{3}\beta^6\gamma^2 + \frac{14}{5}\beta^4\gamma^4$$
$$+ \frac{4}{3}\beta^2\gamma^6 + \frac{1}{9}\gamma^8 \qquad = B_4,$$

&c.

If we write $S_1$ for the sum of all the values of $\zeta^2$,

$S_2$ for the sum of all products such as $\zeta_1^2$, $\zeta_2^2$,

$S_3$ .................................... $\zeta_1^2$, $\zeta_2^2$, $\zeta_3^2$,

then for $r$ terms

$$B_1S_r - B_2S_{r-1} + B_3S_{r-2} - \&c. \ (-)^r B_{r+1} = 0,$$
$$B_2S_r - B_3S_{r-1} + B_4S_{r-2} - \&c. \ (-)^r B_{r+2} = 0,$$
..................................................
$$B_{r+1}S_r - B_{r+2}S_{r-1} + B_{r+3}S_{r-2} - \&c. \ (-)^r B_{2r+1} = 0,$$

a set of $r$ equations, from which the quantities $R_1$, $R_2$ have been eliminated, and from which we may determine the $r$ quantities $S_1 \ldots S_r$, and the values of $\zeta$ are then given as the roots of the equation

$$\zeta^{2r} - S_1\zeta^{2r-2} + S_2\zeta^{2r-4} - \&c. \ (-)^r S_r = 0.$$

Thus if we have three values of $\zeta$ they should be

$$\zeta_0 = 0, \qquad \zeta_1 = \pm [3\beta^4 + 10\beta^2\gamma^2 + 3\gamma^4]^{\frac{1}{2}} [5\beta^2 + 5\gamma^2]^{-\frac{1}{2}},$$

$$R_0 = \frac{4}{3}\,\frac{\beta^4 + 5\beta^2\gamma^2 + \gamma^4}{3\beta^4 + 10\beta^2\gamma^2 + 3\gamma^4}, \qquad R_1 = \frac{5}{6}\,\frac{\beta^4 + 2\beta^2\gamma^2 + \gamma^4}{3\beta^4 + 10\beta^2\gamma^2 + 3\gamma^4}.$$

If $\beta = \gamma = 1$,

$$\zeta_0 = 0, \qquad \zeta_1 = \pm\sqrt{\frac{8}{5}},$$

$$R_0 = \frac{7}{12}, \qquad R_1 = \frac{5}{24}.$$

When the quantity to be integrated is a perfectly general function of the variables we must proceed in a different manner.

We may begin as before by transforming the double integral into one between the limits $\pm 1$ for both variables, so that

$$I = \int_{x_1}^{x_2}\int_{y_1}^{y_2} u\,dx\,dy = \frac{1}{4}(x_2 - x_1)(y_2 - y_1)\int_{-1}^{1}\int_{-1}^{1} u\,dp\,dq \text{.....(1)}.$$

Let $\Sigma(u_n)$ denote the sum of the eight values of $u$ corresponding to the following eight systems of values of $p$ and $q$,

$$(a_n, b_n),\ (a_n, -b_n),\ (-a_n, b_n),\ (-a_n, -b_n);$$

$$(b_n, a_n),\ (b_n, -a_n),\ (-b_n, a_n),\ (-b_n, -a_n),$$

and let us assume that the value of the integral is of the form

$$I = \frac{1}{4}(x_2 - x_1)(y_2 - y_1)\{R_0\Sigma(u_0) + R_1\Sigma(u_1) + \&\text{c.} + R_n\Sigma(u_n)\} \text{...(2)}.$$

The values of the coefficients $R$, $a$ and $b$ are to be deduced from equations formed by equating the sum of the terms in $p^\alpha q^\beta$ in this expression with the integral

$$\int_{-1}^{+1}\int_{-1}^{+1} p^\alpha q^\beta\,dp\,dq = \frac{4}{(\alpha+1)(\beta+1)} \text{..............(3)}.$$

Only those terms in which both $\alpha$ and $\beta$ are even will require to be considered, for the symmetrical distribution of the values of $p$ and $q$ ensures that the terms in which either $\alpha$ or $\beta$ is odd must disappear.

Also since the expression is symmetrical with respect to $p$ and $q$, the term in $p^\beta q^\alpha$ will give an equation identical with that in $p^\alpha q^\beta$.

We may therefore write down the equations at once, leaving out the factor $p^\alpha q^\beta$ common to each term, but writing it at the side to indicate how the equation was obtained. There are $\alpha + 1$ equations in the first group, in which $\beta = 0$,

$$p^0q^0 \qquad \Sigma\,[2R_n] \qquad = 4,$$

$$p^2q^0 \qquad \Sigma\,[R_n(p_n^{\,2}+q_n^{\,2})] \quad = \frac{4}{3},$$

$$p^4q^0 \qquad \Sigma\,[R_n(p_n^{\,4}+q_n^{\,4})] \quad = \frac{4}{5},$$

$$p^{2\alpha}q^0 \qquad \Sigma\,[R_n(p_n^{\,2\alpha}+q_n^{\,2\alpha})] = \frac{4}{2\alpha+1}.$$

There are $\alpha-1$ equations in the second group, in which $\beta=2$,

$$p^2q^2 \qquad \Sigma\,[2R_n\,p_n^{\,2}q_n^{\,2}] \qquad = \frac{4}{3\,.\,3}.$$

$$p^4q^2 \qquad \Sigma\,[R_n\,p_n^{\,2}q_n^{\,2}(p_n^{\,2}+q_n^{\,2})] \qquad = \frac{4}{5\,.\,3}.$$

$$p^{2\alpha-2}q^2 \qquad \Sigma\,[R_n p_n^{\,2}q_n^{\,2}(p^{2\alpha-4}+q^{2\alpha-4})] = \frac{4}{(2\alpha-1)\,.\,3}.$$

There will be $\alpha-3$ equations in which $\beta=4$, and so on. Hence if $\alpha$ is even, the whole number of equations is

$$\left(\frac{\alpha}{2}+1\right)^2.$$

If $\alpha$ is odd, the number is

$$\frac{(\alpha+1)\,(\alpha+3)}{4}.$$

To satisfy these equations we have in general, for each group of values of $u$, three disposable quantities, $R$, $p$ and $q$.

If, however, the central ordinate be selected it will constitute the first group, and will introduce only one disposable quantity, namely $R_0$.

Also, if ordinates lying on the axes of $p$ or of $q$ be chosen, the groups so formed contain only two disposable quantities, one of the ordinates being zero.

Also for ordinates lying on the diagonals, $q=p$, so that for these also there are only two disposable quantities.

Thus if $\alpha=3$, the number of equations is $\frac{4\,.\,6}{4}=6$; and if we select the central ordinate, giving one disposable quantity, a

group of four points on the axes, giving two disposable quantities, and a group of eight points giving three disposable quantities, we shall be able to satisfy the six equations, and to form an expression for the integral which will be correct for any function not exceeding the seventh degree.

We assume

$$\int_{-1}^{1}\int_{-1}^{1} u\,dp\,dq = Pu_o + Q\Sigma(u_{p,o}) + R\Sigma(u_{q,r}) \quad \text{.........(4).}$$

The equations are

$$P + 4Q + 8R = 4,$$

$$Qp^2 + 2R(q^2 + r^2) = \frac{2}{3},$$

$$Qp^4 + 2R(q^4 + r^4) = \frac{2}{5},$$

$$Qp^6 + 2R(q^6 + r^6) = \frac{2}{7},$$

$$2Rq^2r^2 = \frac{1}{9},$$

$$Rq^2r^2(q^2 + r^2) = \frac{1}{15}.$$

The solution of these equations gives

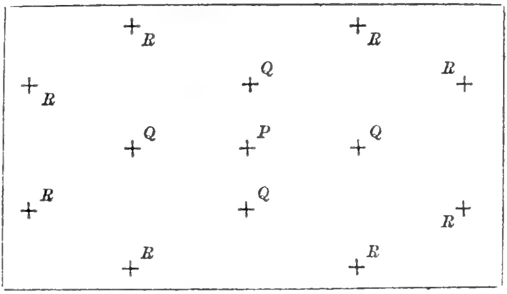
$$p^2 = \frac{12}{35}, \qquad p = \pm 0{\cdot}5855571,$$

$$q^2 = \frac{3}{5}\left[1 + \left(\frac{6}{31}\right)^{\frac{1}{2}}\right], \qquad q = \pm 0{\cdot}9294971,$$

$$r^2 = \frac{3}{5}\left[1 - \left(\frac{6}{31}\right)^{\frac{1}{2}}\right], \qquad r = \pm 0{\cdot}57969554,$$

$$P = \frac{8}{162}, \qquad Q = \frac{98}{162}, \qquad R = \frac{31}{162}.$$

The positions of the thirteen points are given in the annexed figure.




## *Triple Integration.*

In extending this method to triple integration we meet with the curious result, that in certain cases the solution indicates that we are to employ values of the function some of which correspond to values of the variables outside the limits of integration.

Thus if we endeavour to determine twenty-seven sets of values of $x$, $y$ and $z$, with a corresponding set of multipliers, so as to express the value of the triple integral in the form

$$\int_{-a}^{a}\int_{-b}^{b}\int_{-c}^{c} u\,dx\,dy\,dz = abc\,\{Ou_0 + P\Sigma(u_p) + Q\Sigma(u_q) + R\Sigma(u_r)\}\ldots(1),$$

where $u_0$ denotes the value of $u$ when $x = y = z = 0$;

$\Sigma(u_p)$ denotes the sum of the six values of $u$ for which

$$\begin{array}{lll} x = \pm pa, & 0, & 0, \\ y = 0, & \pm pb, & 0, \\ z = 0, & 0, & \pm pc; \end{array}$$

$\Sigma(u_q)$ denotes the sum of the twelve values of $u$ for which

$$\begin{array}{lll} x = 0, & \pm qa, & \pm qa, \\ y = \pm qb, & 0, & \pm qb, \\ z = \pm qc, & \pm qc, & 0; \end{array}$$

and $\Sigma(u_r)$ denotes the sum of the eight values of $u$ for which

$$\begin{array}{l} x = \pm ra, \\ y = \pm rb, \\ z = \pm rc, \end{array}$$

where the signs may be taken in any order.

The equations to be satisfied in order that equation (1) may be satisfied for any function of $x$, $y$ and $z$ of not more than seven dimensions are

$$O + 6P + 12Q + 8R = 8,$$

$$2Pp^2 + 8Qq^2 + 8Rr^2 = \frac{8}{3},$$

$$2Pp^4 + 8Qq^4 + 8Rr^4 = \frac{8}{5},$$

$$2Pp^6 + 8Qq^6 + 8Rr^6 = \frac{8}{7},$$

$$4Qq^4 + 8Rr^4 = \frac{8}{9},$$

$$4Qq^6 + 8Rr^6 = \frac{8}{15},$$

$$8Rr^6 = \frac{8}{27}.$$

The solution of these equations gives two systems of values:

*First System.*

$$\frac{1}{p_1^2} = 1 + \left(\frac{5}{33}\right)^{\frac{1}{2}},$$

$$\frac{1}{q_1^2} = \frac{5}{7}\left[2 - \left(\frac{11}{15}\right)^{\frac{1}{2}}\right],$$

$$\frac{1}{r_1^2} = \frac{1}{7}\left[13 + 4\left(\frac{11}{15}\right)^{\frac{1}{2}}\right],$$

$p_1 = 0{\cdot}969773,$

$q_1 = 1{\cdot}119224,$

$r_1 = 0{\cdot}652817,$

$O_1 = -2{\cdot}856446,$

$P_1 = \quad 1{\cdot}106789,$

$Q_1 = \quad 0{\cdot}032304,$

$R_1 = \quad 0{\cdot}478508.$

*Second System.*

$$\frac{1}{p_2^2} = 1 - \left(\frac{5}{33}\right)^{\frac{1}{2}}.$$

$$\frac{1}{q_2^2} = \frac{5}{7}\left[2 + \left(\frac{11}{15}\right)^{\frac{1}{2}}\right].$$

$$\frac{1}{r_2^2} = \frac{1}{7}\left[13 - 4\left(\frac{11}{15}\right)^{\frac{1}{2}}\right].$$

$p_2 = 2{\cdot}638430.$

$q_2 = 0{\cdot}602526.$

$r_2 = 0{\cdot}855044.$

$O_2 = -7{\cdot}999462.$

$P_2 = \quad 0{\cdot}000513.$

$Q_2 = \quad 1{\cdot}238514.$

$R_2 = \quad 0{\cdot}094777.$

In the first system $q_1$ is greater than unity, and in the second system $p_2$ is greater than unity, so that in either case one set of the values of $u$ corresponds to values of the variables outside of the limits of integration.

This, of course, renders the method useless in determining the integral from the *measured* values of the quantity $u$, as when we wish to determine the weight of a brick from the specific gravities of samples taken from 27 selected places in the brick, for we are directed by the method to take some of the samples from places outside the brick.

But this is not the case contemplated in the mathematical enunciation. All that we have proved is that if $u$ be a function of $x$, $y$, $z$ of not more than seven dimensions, our method will lead to a correct value, and of course we can determine the value of such a function for any values of the variables, whether they lie within the limits of integration or not.

(3) Mr J. W. L. Glaisher, M.A., F.R.S. *Preliminary account of an enumeration of the primes in Burckhardt's tables* (1 *to* 3,000,000.)

The present paper is a continuation of that published in pp. 17—23 of this volume, and relates to the enumeration of the primes between 1 and 3,000,000, the enumeration being made from Burckhardt's *Tables des diviseurs* (Paris, 1814—1817). The work was continued regularly upon the printed forms described on p. 20, and was performed with such care that on comparing the new calculation with the old duplicate calculation, the former was found to be almost wholly free from error: in fact, two millions were quite correct, and only two errors were found in the third million. As for the old calculation, the first million was free from error[1]; but several errors were found in the second and third millions. Considering that the new calculation was performed by a very careful computer, who had had no connexion with the earlier work, and that the old calculation was itself the result of at least two independent enumerations, I feel very little doubt that the numbers given in the present paper may be depended upon with confidence.

[1] The old enumeration was made from Chernac's *Cribrum Arithmeticum* (1811); the new one from Burckhardt's tables, and all the discrepancies between the two were due to errors in Chernac which are noted by Burckhardt on the first page of the preface to his First Million (1817).

The results of the enumeration are shown in Tables *A*, *B*, *C* (at the end of the paper), which are arranged in an exactly similar manner to Tables I, II, III (pp. 21—23): the explanation of the mode of arrangement is given on p. 19.

The numbers of primes in each quarter million up to three millions are:—

| | First Million. | Second Million. | Third Million. |
|---|---|---|---|
| First quarter | 22,045 | 17,971 | 17,150 |
| Second ,, | 19,494 | 17,682 | 16,991 |
| Third ,, | 18,700 | 17,455 | 16,922 |
| Fourth ,, | 18,260 | 17,325 | 16,822 |
| Total............... | 78,499 | 70,433 | 67,885 |

and these may be compared with the similar values for the three millions from 3,000,000 to 6,000,000 on p. 19.

It will be seen that in the first million 7·8 per cent. of the numbers are primes, in the second million 7·0 per cent., and in the third 6·8, while in each of the seventh, eighth, and ninth millions the percentage is about 6·3. In the first hundred thousand numbers about 10 per cent. are primes, and this percentage is reduced to 6·7 at the end of the third million, and to 6·3 at the end of the ninth million. Throughout the whole of the first million the numbers of primes in each group of 100,000 steadily diminish, but this is not the case for the second or third millions, or for any one of the other three millions.

The numbers of primes in each of the six millions are:

| | Number of Primes. | Difference. |
|---|---|---|
| First million | 78,499 | |
| | | 8,066 |
| Second ,, | 70,433 | |
| | | 2,548 |
| Third ,, | 67,885 | |
| Seventh ,, | 63,799 | |
| | | 641 |
| Eighth ,, | 63,158 | |
| | | 398 |
| Ninth ,, | 62,760 | |

The difference between the numbers of primes in the third and seventh millions is 4,086, giving for each of the four intermediate differences an average of $1021\frac{1}{2}$.

Several enumerations have been made of the primes in the first million. In vol. ii. of the third edition of his *Théorie des Nombres* (p. 65), Legendre gave the number of primes in the first million as 78,493, which differs from the true value by 6. The following Table contains the results of the enumeration as given

by Legendre, and the values found, with the same limits, by the present enumeration: it will be seen that the comparatively small discrepancy of 6 is due to the fact that several of the larger errors compensate one another very nearly.

| Limits. | Number of Primes | |
|---|---|---|
| | Counted by Legendre. | True value. |
| 0— 10,000 | 1,230 | 1,230 |
| 10,000— 20,000 | 1,033 | 1,033 |
| 20,000— 30,000 | 983 | 983 |
| 30,000— 40,000 | 958 | 958 |
| 40,000— 50,000 | 930 | 930 |
| 50,000— 60,000 | 924 | 924 |
| 60,000— 70,000 | 878 | 878 |
| 70,000— 80,000 | 901 | 902 |
| 80,000— 90,000 | 876 | 876 |
| 90,000— 100,000 | 879 | 879 |
| 100,000— 150,000 | 4,257 | 4,256 |
| 150,000— 200,000 | 4,135 | 4,136 |
| 200,000— 250,000 | 4,061 | 4,060 |
| 250,000— 300,000 | 3,943 | 3,953 |
| 300,000— 350,000 | 3,989 | 3,980 |
| 350,000— 400,000 | 3,884 | 3,883 |
| 400,000— 500,000 | 7,677 | 7,678 |
| 500,000— 600,000 | 7,555 | 7,560 |
| 600,000— 700,000 | 7,442 | 7,445 |
| 700,000— 800,000 | 7,402 | 7,408 |
| 800,000— 900,000 | 7,331 | 7,323 |
| 900,000—1,000,000 | 7,225 | 7,224 |
| 0—1,000,000 | 78,493 | 78,499 |

In vol. ii. of Gauss's *Werke* (Göttingen, 1863), pp. 436—443, there are given in detail the results of an enumeration of the primes in the first three millions: a letter from Gauss to Encke, dated December 24, 1849, which is published on pp. 444—447 contains an account of the manner in which the enumeration was performed. It was commenced by Gauss as early as 1792 or 1793, when he first obtained a copy of Lambert's *Supplementa*, which contains a list of primes up to 102,000. Subsequently when Vega's *Tabulæ* of 1796[1] appeared he was able to extend the enumeration to 400,031.

In 1811 Chernac's *Cribrum Arithmeticum* was published, to the great joy of Gauss, who states that, although he had not

[1] I have seen two copies of Vega's *Tabulæ*, and they both bear the date 1797. Probably Gauss was writing from memory; but it is possible that some copies of the *Tabulæ* may have been circulated at the end of 1796. The preface is dated February 1, 1797.

sufficient patience for a continuous enumeration of the whole million, he often employed unoccupied quarters of an hour in counting here and there a chiliad. Finally the work was laid aside without the million being made complete, but later on, Gauss "made use of Goldschmidt's industry, to fill in the gaps that were left in the first million, and to extend the enumeration further by means of Burckhardt's Tables." Dr Schering, in his notes at the end of the volume (p. 520) states that the Table for the first million was in Gauss's own handwriting, and that the Tables for the second and third millions were in Goldschmidt's handwriting. It is thus clear that the enumeration could not be a very accurate one: in fact, the books used were themselves by no means free from error. Gauss states that the chiliad in Lambert's *Supplementa* between 101,000 and 102,000 "swarmed with errors;" seven composite numbers in this chiliad were given as primes, and two primes were omitted. Also Chernac at the end of his *Cribrum* gives a list of 20 composite numbers that are contained in Vega's list of primes, and of 22 primes that are omitted. Gauss did not himself publish his results, and was, no doubt, perfectly aware that they were not to be depended upon as accurate.

In the first million (in which the enumeration was made chiefly by Gauss himself), the number of primes in each chiliad is given; but the results for the other two millions are arranged in a similar manner to Tables *A*, *B*, *C* at the end of the present paper. There are, in all, 22 such Tables, one for each group of 100,000 in the second and third millions, and one for the whole of the second million similar to *B*, and one for the third million similar to *C*: this part of the enumeration was made, as mentioned above, by Goldschmidt.

When, in 1873, I compared my results with the Tables in Gauss's *Werke*, I found many discrepancies: but in the notes to the new edition of the *Werke* (1876) nineteen errata, found by Dr Meissel, of Iserlohn, are pointed out in Gauss's first million, and when these are corrected the values agree entirely with my own, except in one instance, viz. the number of primes in the 354$^{\text{th}}$ chiliad should be 76 instead of 79.

Recently, since the completion of my enumeration, Professor H. J. S. Smith called my attention to Meissel's paper, "Ueber die Bestimmung der Primzahlenmenge innerhalb gegebener Grenzen" (*Mathematische Annalen*, t. ii. (1870), pp. 636—642), in which are given the errata in the first million that are reproduced in the second edition of Gauss's *Werke*. I find that the error in the 354$^{\text{th}}$ chiliad is not noticed by Meissel in his paper, but as he assigns 7,863 as the number of primes between the 300$^{\text{th}}$ and the

400th chiliad, which agrees with the number in Table *A*, it is clear that he must have used the correct value in his calculation. Meissel gives the number of primes in each group of 100,000 in the first million, and his values agree with those in Table *A*. In the first 100,000 however, he gives the number of primes as 9,592, while the number in Table *A* is 9,593; but this discrepancy is no doubt due to the fact that he has not counted 1 and 2 as primes, for in Gauss's Table the number of primes in the first chiliad is given as 168, and Meissel accepts this value; but if 1 and 2 be both counted as primes the number is 169. Meissel obtained the number of primes in the first million both by counting from Burckhardt's Tables, and also by means of an analytical process of his own, and as the results obtained by him agree with those given in this paper, there can be no doubt of the accuracy of the enumeration as far as the first million is concerned. It does not seem that Meissel has given any results relating to the counting of primes from Burckhardt's or Dase's Tables in the second or higher millions; but by his analytical process he has calculated the number of primes in the first ten millions, and also in the first hundred millions[1].

It would occupy too much space to give in detail the errata in the Tables for the second and third millions that appear in Gauss's *Werke*, but the following list contains the values given in Gauss for the number of primes in each group of 100,000 and the values found by the present enumeration.

*Second Million.*

| Limits. | Number of Primes. | |
|---|---|---|
| | Gauss. | Table *B*. |
| 1,000,000—1,100,000 | 7,210 | 7,216 |
| 1,100,000—1,200,000 | 7,194 | 7,225 |
| 1,200,000—1,300,000 | 7,081 | 7,081 |
| 1,300,000—1,400,000 | 7,098 | 7,103 |
| 1,400,000—1,500,000 | 7,028 | 7,028 |
| 1,500,000—1,600,000 | 6,971 | 6,973 |
| 1,600,000—1,700,000 | 7,012 | 7,015 |
| 1,700,000—1,800,000 | 6,931 | 6,932 |
| 1,800,000—1,900,000 | 6,955 | 6,957 |
| 1,900,000—2,000,000 | 6,902 | 6,903 |
| 1,000,000—2,000,000 | 70,382 | 70,433 |

[1] "Berechnung der Menge von Primzahlen, welche innerhalb der ersten Hundert Millionen natürlicher Zahlen vorkommen." *Mathematische Annalen*, t. iii. (1871), pp. 523—525.

*Third Million.*

| Limits. | Number of Primes. | |
|---|---|---|
| | Gauss. | Table *C.* |
| 2,000,000—2,100,000 | 6,874 | 6,874 |
| 2,100,000—2,200,000 | 6,857 | 6,857 |
| 2,200,000—2,300,000 | 6,849 | 6,849 |
| 2,300,000—2,400,000 | 6,787 | 6,791 |
| 2,400,000—2,500,000 | 6,766 | 6,770 |
| 2,500,000—2,600,000 | 6,804 | 6,809 |
| 2,600,000—2,700,000 | 6,762 | 6,765 |
| 2,700,000—2,800,000 | 6,714 | 6,716 |
| 2,800,000—2,900,000 | 6,744 | 6,746 |
| 2,900,000—3,000,000 | 6,705 | 6,708 |
| 2,000,000—3,000,000 | 67,862 | 67,885 |

In my previous paper (p. 20) I remarked that the numbers given in the "primes counted" column of my Table in the British Association Report for 1872 were completely verified for the ninth million by the subsequent examination of that million: but I find that there are several errors in the numbers in the "primes counted" column for the second million which appear in the same Table: they are as follows:—

| Limits. | Number of Primes. | |
|---|---|---|
| | Value given in 1872. | True value. |
| 1,000,000—1,050,000 | 3,635 | 3,636 |
| 1,350,000—1,400,000 | 3,579 | 3,581 |
| 1,400,000—1,450,000 | 3,501 | 3,502 |
| 1,600,000—1,650,000 | 3,498 | 3,508 |
| 1,700,000—1,750,000 | 3,468 | 3,467 |

and the total number of primes in the million is 70,433 instead of 70,420.

There are two papers by Hargreave in the Philosophical Magazine for 1849 and 1854 that relate to the distribution of primes, viz. "Analytical researches concerning numbers" (Vol. xxxv., pp. 36—53) and "On the law of prime numbers" (Ser. 4, vol. viii., pp. 114—122). It does not appear that Hargreave made an enumeration of the whole of the first million, though he seems to have counted the primes in some portions of it: the number of primes for the whole million is taken from Legendre. In the second paper he states that he has found, by counting, the number of primes between 2,000,000 and 3,000,000 to be 67,751, and he also

explains a process of calculating the number of primes up to any limit, which applied to the first million gives 78,494 as the number of primes. A determination is also given of the number of primes in the first ten millions.

In two copies of Burckhardt's Tables (and therefore very likely in all) there is no mark whatever to be seen corresponding to the number 2,882,699: this is no doubt caused by the type having slipped back. On commencing the investigation of whether this number was or was not a prime, it at once appeared that it was divisible by 19.

In Table I of my previous paper it was assumed that 6,036,637[1] was prime. This is correct. I have had the number divided by all the primes up to 2,457 by two computers independently, and have compared the separate divisions: in all cases there was a remainder.

In this preliminary account I have confined myself entirely to the chief results of the enumeration, and have given no comparisons with the li$x$ formula. A good many values of the integral are already calculated, but it seems desirable to reserve any discussion of the agreement between the primes counted and the formulæ till the comparisons can be given in a complete form.

[1] This number is correctly given in line 5 of p. 20, but in line 8 it is misprinted 6,037,637. Also, in line 6 of p. 18, t. III. should be t. II.

## TABLE *A*.

0 to 1,000,000.

| $n$ | Number of centuries each of which contains $n$ primes. | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0 to 100,000 | 100,000 to 200,000 | 200,000 to 300,000 | 300,000 to 400,000 | 400,000 to 500,000 | 500,000 to 600,000 | 600,000 to 700,000 | 700,000 to 800,000 | 800,000 to 900,000 | 900,000 to 1,000,000 | 0 to 1,000,000 |
| 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 1 | 0 | 1 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 3 |
| 2 | 0 | 0 | 0 | 2 | 4 | 3 | 7 | 5 | 4 | 4 | 29 |
| 3 | 3 | 7 | 13 | 12 | 13 | 20 | 16 | 19 | 21 | 16 | 140 |
| 4 | 6 | 15 | 24 | 40 | 39 | 44 | 43 | 53 | 53 | 55 | 372 |
| 5 | 18 | 47 | 55 | 72 | 91 | 87 | 105 | 97 | 114 | 115 | 801 |
| 6 | 39 | 116 | 134 | 127 | 153 | 142 | 145 | 162 | 165 | 179 | 1,362 |
| 7 | 106 | 145 | 173 | 172 | 175 | 193 | 208 | 193 | 182 | 218 | 1,765 |
| 8 | 149 | 184 | 202 | 200 | 189 | 185 | 173 | 194 | 176 | 169 | 1,821 |
| 9 | 185 | 196 | 167 | 171 | 144 | 153 | 155 | 119 | 145 | 119 | 1,554 |
| 10 | 177 | 146 | 131 | 111 | 86 | 110 | 78 | 79 | 76 | 64 | 1,058 |
| 11 | 124 | 78 | 59 | 49 | 63 | 40 | 47 | 54 | 44 | 34 | 592 |
| 12 | 104 | 42 | 29 | 29 | 33 | 16 | 16 | 17 | 13 | 17 | 316 |
| 13 | 46 | 18 | 7 | 11 | 7 | 7 | 6 | 6 | 5 | 9 | 122 |
| 14 | 17 | 3 | 5 | 2 | 2 | 0 | 1 | 0 | 2 | 0 | 32 |
| 15 | 14 | 2 | 0 | 2 | 0 | 0 | 0 | 1 | 0 | 1 | 20 |
| 16 | 8 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 8 |
| 17 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 3 |
| 21 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| 26 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| No. of primes | 9,593 | 8,392 | 8,013 | 7,863 | 7,678 | 7,560 | 7,445 | 7,408 | 7,323 | 7,224 | 78,499 |

The last line in each column shows the number of primes in the group to which the column has reference.

[*Note.* In this table 1 and 2 are counted as primes.]

# TABLE *B*.

1,000,000 to 2,000,000.

| $n$ | Number of centuries each of which contains $n$ primes. | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1,000,000 to 1,100,000 | 1,100,000 to 1,200,000 | 1,200,000 to 1,300,000 | 1,300,000 to 1,400,000 | 1,400,000 to 1,500,000 | 1,500,000 to 1,600,000 | 1,600,000 to 1,700,000 | 1,700,000 to 1,800,000 | 1,800,000 to 1,900,000 | 1,900,000 to 2,000,000 | 1,000,000 to 2,000,000 |
| 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 |
| 1 | 1 | 1 | 2 | 1 | 5 | 2 | 2 | 1 | 0 | 1 | 16 |
| 2 | 4 | 5 | 6 | 9 | 7 | 10 | 11 | 5 | 10 | 5 | 72 |
| 3 | 21 | 23 | 32 | 19 | 19 | 28 | 29 | 30 | 22 | 34 | 257 |
| 4 | 53 | 58 | 63 | 69 | 72 | 77 | 67 | 70 | 71 | 67 | 667 |
| 5 | 115 | 102 | 120 | 119 | 130 | 124 | 121 | 152 | 135 | 135 | 1,253 |
| 6 | 170 | 170 | 160 | 172 | 181 | 198 | 159 | 174 | 176 | 183 | 1,743 |
| 7 | 217 | 220 | 214 | 207 | 180 | 172 | 203 | 194 | 204 | 221 | 2,032 |
| 8 | 164 | 156 | 168 | 162 | 183 | 150 | 174 | 146 | 160 | 149 | 1,612 |
| 9 | 125 | 136 | 111 | 118 | 98 | 124 | 129 | 125 | 115 | 101 | 1,182 |
| 10 | 73 | 76 | 73 | 72 | 73 | 63 | 63 | 61 | 74 | 63 | 691 |
| 11 | 39 | 35 | 35 | 33 | 34 | 29 | 27 | 26 | 23 | 30 | 311 |
| 12 | 12 | 10 | 9 | 15 | 16 | 17 | 9 | 10 | 10 | 5 | 113 |
| 13 | 6 | 6 | 5 | 3 | 2 | 6 | 3 | 5 | 0 | 3 | 39 |
| 14 | 0 | 2 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 3 | 7 |
| 15 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 0 | 3 |
| 16 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| No. of primes | 7,216 | 7,225 | 7,081 | 7,103 | 7,028 | 6,973 | 7,015 | 6,932 | 6,957 | 6,903 | 70,433 |

The last line in each column shows the number of primes in the group to which the column has reference.

## TABLE *C.*

2,000,000 to 3,000,000.

| $n$ | Number of centuries each of which contains $n$ primes. | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 2,000,000 to 2,100,000 | 2,100,000 to 2,200,000 | 2,200,000 to 2,300,000 | 2,300,000 to 2,400,000 | 2,400,000 to 2,500,000 | 2,500,000 to 2,600,000 | 2,600,000 to 2,700,000 | 2,700,000 to 2,800,000 | 2,800,000 to 2,900,000 | 2,900,000 to 3,000,000 | 2,000,000 to 3,000,000 |
| 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 |
| 1 | 3 | 2 | 2 | 4 | 1 | 3 | 4 | 2 | 2 | 2 | 25 |
| 2 | 10 | 9 | 9 | 10 | 9 | 5 | 10 | 7 | 15 | 13 | 97 |
| 3 | 32 | 27 | 29 | 33 | 37 | 35 | 28 | 43 | 30 | 44 | 338 |
| 4 | 69 | 70 | 73 | 86 | 78 | 88 | 70 | 93 | 84 | 64 | 775 |
| 5 | 119 | 145 | 138 | 135 | 146 | 136 | 159 | 137 | 141 | 152 | 1,408 |
| 6 | 197 | 183 | 179 | 177 | 193 | 193 | 195 | 195 | 179 | 187 | 1,878 |
| 7 | 204 | 201 | 205 | 194 | 190 | 178 | 201 | 188 | 222 | 214 | 1,997 |
| 8 | 157 | 167 | 168 | 157 | 151 | 173 | 141 | 145 | 132 | 135 | 1,526 |
| 9 | 115 | 110 | 113 | 113 | 102 | 88 | 96 | 87 | 109 | 103 | 1,036 |
| 10 | 63 | 52 | 44 | 54 | 56 | 57 | 54 | 67 | 53 | 58 | 558 |
| 11 | 21 | 18 | 30 | 29 | 25 | 25 | 22 | 24 | 18 | 15 | 227 |
| 12 | 8 | 9 | 10 | 7 | 7 | 13 | 17 | 9 | 7 | 11 | 98 |
| 13 | 2 | 4 | 0 | 1 | 5 | 6 | 1 | 2 | 6 | 1 | 28 |
| 14 | 0 | 3 | 0 | 0 | 0 | 0 | 1 | 0 | 2 | 0 | 6 |
| 15 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 |
| 16 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 17 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 |
| No. of primes | 6,874 | 6,857 | 6,849 | 6,791 | 6,770 | 6,809 | 6,765 | 6,716 | 6,746 | 6,708 | 67,885 |

The last line in each column shows the number of primes in the group to which the column has reference.

# PROCEEDINGS

OF THE

# Cambridge Philosophical Society.

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*April* 23, 1877.

PROF. J. CLERK MAXWELL, F.R.S., PRESIDENT, IN THE CHAIR.

A communication was made to the Society by

MR ARTHUR SCHUSTER, Ph.D., *On the passage of electricity through gases.*

The spectroscope has hitherto done very little in clearing up the important questions connected with the discharge of electricity through gases. The reason of this must be looked for in the difficulty which experimenters have found in settling the chemical origin of the spectra presented by gases which have become luminous through the electric discharge. It was also some time before spectroscopists perceived clearly the meaning of the different spectra seen sometimes in one and the same tube. The ground is beginning to clear now, and I propose to shew in this paper how the spectroscope may be made useful in study of the passage of electricity through gases.

Faraday has made the remark, that the differences known to exist between the negative and positive pole of a discharge of electricity through a gaseous medium may be due to secondary chemical causes, such as produce similar phenomena in some electrolytes. If this is true we should expect to find different spectra at the two poles; for the secondary chemical causes referred to by Faraday consist in the formation of chemical compounds at the poles, and we know that each compound has its own spectrum. It has long been known that nitrogen gives a spectrum at the negative pole which under ordinary circumstances is seen in no

other part of the tube. Wullner has proved that the same holds for oxygen. I have been able to confirm it in the case of carbonic oxide and most likely in the case of chlorine. In the latter case I have not been able as yet to decide whether the observed difference is not due to a mere difference in temperature. In all the other cases it is certain that no differences in temperature or pressure can produce the change in the spectrum. There is no more doubt that the different spectra of one and the same gas are produced by structural differences in the vibrating molecule. These structural differences are produced in most, if not in all cases, by a difference in the number of atoms making up the molecule. If it can be shewn therefore that no mere difference in temperature or pressure can produce the molecular structure which gives rise to the spectrum at the negative pole, it must be due to chemical or other causes operating at the negative pole only of a vacuum tube. This proof I propose to give.

In a great many cases the introduction of a Leyden Jar alters the character of the spectrum. In all these gases, even without the Jar, traces of the Jar-spectrum are seen in the negative pole. If therefore temperature is the only operative cause, the temperature at the negative pole must be intermediate between that in the capillary part with and without the Jar, for without the Jar the traces of the Jar-spectrum, if seen at all, are seen much weaker in the capillary part than at the negative pole. By a suitable intensity of discharge, a state can be established in which the Jar-spectrum is seen in the capillary part of a Geissler-tube, while the wide part of the tube still retains the spectrum of the weaker discharge. I have examined such a tube containing nitrogen, and projected the image of the neck of the capillary part on the slit of a spectroscope by means of a lens. I could see in the spectroscope at the same time the spectrum of the capillary part and that of the wide part. The appearance was as follows. The capillary part shewed the line-spectrum of nitrogen, and the lines gradually faded away towards the wide part. On the other hand, the wide part shewed the bands of nitrogen which gradually faded away towards the capillary part. The intermediate part was therefore filled up with the longest lines of the line-spectrum and the longest bands of the band-spectrum. One cross-section could be found, in which just those lines of the Jar-spectrum, which are seen at the negative pole, were present. If temperature was the only operative cause, the bands characteristic of the negative pole ought to be observed at that point, but neither at that nor any other point intermediate between the line and band-spectrum could the spectrum of the negative pole be seen. This can be observed through a wide range of pressure, so that the pressure also may be eliminated as operative cause. If it is thus conclusively established that

special causes must be at operation at the negative pole of a discharge through a gas, it is no more astonishing that traces of the simpler spectrum of the jar-discharge are seen at the negative pole. For it is probable that the combination which gives rise to the spectrum of the negative pole is due to the action of the simple atom giving the line-spectrum, which is set free at the electrode. I wish to draw attention to the fact that ozone is chiefly formed at the negative pole, and that though we know that ozone is destroyed by temperature, we have no right to suppose that temperature will destroy all similar combinations if formed.

There is one observation which I believe to be of sufficient interest to be recorded, because it shews that there is no real difference between the spark-discharge at ordinary pressures and what we call a continuous discharge in gases. If a spark is sent through oxygen at ordinary pressures, the line-spectrum is seen, but if the intensity of the spark is reduced below a certain point, the spark takes a yellow colour, and a purple point is seen at the negative pole. The yellow spark shews a continuous spectrum, while the purple point shews the spectrum of the negative pole. The whole spark has in the spectroscope the same appearance as the continuous discharge in a Geissler tube.

It is no argument against the view which I have brought forward, that the spectrum of the negative pole is seen under certain circumstances at places removed from the metallic electrode. Faint traces of the spectrum are sometimes seen throughout the tube, and under very great exhaustion it is always seen even in the capillary part. The diffusion of gases sufficiently accounts for this fact, which would be difficult to explain if we did not assume that the spectrum is due to a distinct molecular combination.

Again, Mr Goldstein has observed that at all places where negative electricity comes out of a narrow into a wide part of a tube, a glow is seen which in all its properties resembles the glow surrounding the negative electrode, and the spectrum of the negative pole is seen at that place. The causes which produce the combination give rise to that spectrum, and must therefore be in operation at all points in which there is rapid increase in cross-section in the direction of the negative current.

The most difficult part of the problem of the conductivity of electricity through gases is the passage of electricity from the electrode to the gas. The important part which the electrode plays has long been known. The negative electrode especially is disintegrated, and parts of it are deposited on the glass-wall surrounding the electrode. Vacuum tubes, owing to long-continued use, shew sometimes remarkable changes. Mr Goldstein has found that these changes are due to the absorption of the gas by the

electrode. If, after the absorption, the electrodes are heated from outside, the gas is given off again and not reabsorbed until discharges of electricity are sent through the tube. These discharges cause the gradual reabsorption of the gas.

The following observation is of interest in connexion with this subject. One of the electrodes of a Geissler tube containing oxygen was accidentally covered with minute specks of carboniferous matter. The negative glow in oxygen has a purple colour, in carbonic oxide it is blue. In the tube in question the purple glow of oxygen was traversed by blue lines of flow, extending outwards from the negative electrode into the surrounding space. Along these lines of flow the carbon therefore was carried away from the electrode. The sharp outline of the line of flow shewed that the carbonic oxide, giving rise to the blue colour, did not diffuse into the oxygen, and *vice versa*, so that the fresh supply of oxygen can only have come from the other side along the stream-line.

A tube containing either a hydrocarbon or carbonic oxide will always shew the spectrum of carbon, in addition to the other spectra seen, though the compound may not be decomposed. The spectrum of carbon however is different in the two cases, being a line-spectrum in one case, and a band-spectrum in the other. This admits of an easy explanation, but only on the supposition that the different spectra of one element are due to different molecular combinations. According to this view it is only natural that the carbon atom dissociated out of carbonic oxide or carbonic acid should give a simpler spectrum, than the carbon separated out of hydrocarbons which contain more than one atom of carbon, bound together.

In conclusion, I may mention an observation which, though not directly connected with this subject, is of interest. Stratifications are sometimes seen in one tube, which shew different colours on different sides. I have projected the image of such a stratification on the slit of a spectroscope. The part of the stratification which was turned towards the positive electrode was pink, and shewed a spectrum the origin of which is not quite settled. It is the so-called band-spectrum of hydrogen, which however, according to Salet, is due to a hydrocarbon. The part of the stratification which was turned towards the negative metallic electrode was blue and its spectrum is that of carbonic oxide. The tube some time before did not shew any stratifications. These came in owing to the gradual absorption of the gas by the electrode. The spectrum seen in the pink part of the stratification was then seen in the capillary part, while the spectrum of the blue parts was seen in the wider part of the tube. A mere difference in temperature in different

parts of the stratification therefore would account for the different spectra. I am unable to say why a difference of temperature should produce the change, but there is as yet no reason to suppose that other causes but those of temperature determine the different spectra in wide and narrow parts of the tube. It would therefore seem that the temperature of that part of the stratification which is turned towards the positive metallic electrode is at a higher temperature than that which is turned away from it, and this agrees with the observations of Mr Goldstein, who found that the stratifications were always brightest at their negative pole, that is, the side which is turned towards the metallic positive electrode.

## *May* 7, 1877.

PROF. CLERK MAXWELL, F.R.S., PRESIDENT, IN THE CHAIR.

A communication was made to the Society by

Mr J. W. L. GLAISHER, M.A., F.R.S. *On expressions for the theta functions as definite integrals.*

(*Abstract.*)

§ 1. THE function $\Theta(x)$ is defined by the equation

$$\Theta\left(\frac{2Kx}{\pi}\right) = 1 - 2q\cos 2x + 2q^4\cos 4x - 2q^9\cos 6x + \&c. \ldots (1),$$

so that the object of the memoir is to express the series on the right-hand side of (1) as a definite integral.

The subject is alluded to by Kummer in his paper, *De integralibus definitis et seriebus infinitis* (Crelle's Journal, t. xvii. pp. 210—242). He there gives the values of the series

$$1 + q + q^4 + q^9 + q^{16} + \&c.$$

and

$$1 - q + q^4 - q^9 + q^{16} - \&c.$$

as definite integrals, and remarks that the same method would enable us to find the values of the series for $\Theta\left(\frac{2Kx}{\pi}\right)$ and $H\left(\frac{2Kx}{\pi}\right)$, 'quarum vero expressiones per integralia definita, quam minus simplices evadant, hoc loco omittimus.'

§ 2. At first sight it seems as if the summation could be at once effected by means of the integral

$$\int_0^\infty e^{-t^2} \cos 2at\, dt = \tfrac{1}{2}\sqrt{\pi}.e^{-a^2},$$

but, putting $q = e^{-a^2}$, the right-hand side of (1) becomes

$$\frac{2}{\sqrt{\pi}}\int_0^\infty e^{-t^2}(1 - \cos 2x \cos 2at + \cos 4x \cos 4at - \&c.)\, dt \quad ...(2),$$

and the value of the quantity in brackets, under the integral sign, is indeterminate, so that the method does not give an expression for the series as a definite integral. In point of fact, as was shown by Cauchy, the result to which (2) does lead, when evaluated, is the transformation (3) used further on.

§ 3. The difficulty was avoided by Kummer as follows. In order to sum the series $1 \pm q + q^4 \pm q^9 + \&c.$, he starts with the summation

$$\cos 2\beta at + v \cos 2(\beta - 1)\, at + v^2 \cos 2(\beta - 2)\, at + \&c.$$
$$= \frac{\cos 2\beta at - v \cos 2(\beta + 1)\, at}{1 - 2v \cos 2at + v^2},$$

whence, $q$ being as before equal to $e^{-a^2}$, we find

$$q^{\beta^2} + vq^{(\beta-1)^2} + v^2 q^{(\beta-2)^2} + \&c.$$
$$= \frac{2}{\sqrt{\pi}}\int_0^\infty e^{-t^2} \frac{\cos 2\beta at - v \cos 2(\beta + 1)\, at}{1 - 2v \cos 2at + v^2}\, dt.$$

The general term of the series is $v^n q^{(\beta-n)^2}, = v^n q^{\beta^2+n^2-2\beta n}$, which $= z^n q^{\beta^2+n^2}$ if $z = vq^{-2\beta}$, and therefore

$$1 + zq + z^2q^4 + z^3q^9 + \&c.$$
$$= \frac{2q^{-\beta^2}}{\sqrt{\pi}}\int_0^\infty e^{-t^2} \frac{\cos 2\beta at - zq^{2\beta} \cos 2(\beta + 1)\, at}{1 - 2zq^{2\beta} \cos 2at + z^2 q^{4\beta}}\, dt,$$

in which $\beta$ is arbitrary, subject only to the condition that $zq^{2\beta}$ must be less than unity. Kummer puts $\beta = \frac{1}{2}$, so that, taking $z = 1$,

$$1 + q + q^4 + q^9 + \&c. = \frac{2}{q^{\frac{1}{4}}\sqrt{\pi}}\int_0^\infty e^{-t^2} \frac{\cos at - q \cos 3at}{1 - 2q \cos 2at + q^2}\, dt.$$

§ 4. On applying a similar method to the theta series, I find that

$$1+\Theta\left(\frac{2Kx}{\pi}\right)$$
$$=\frac{2}{\sqrt{\pi}}q^{-\beta^2}\int_0^\infty e^{-t^2}\left\{\frac{\cos 2\beta at+q^{2\beta t}\cos\{2(\beta+1)at+2x\}}{1+2q^{2\beta t}\cos(2at+2x)+q^{4\beta t}}\right.$$
$$\left.+\frac{\cos 2\beta at+q^{2\beta t}\cos\{2(\beta+1)at-2x\}}{1+2q^{2\beta t}\cos(2at-2x)+q^{4\beta t}}\right\}dt,$$

where $\beta$ is arbitrary, and $q=e^{-a^2}=e^{-\frac{\pi K'}{K}}$.

Thus, putting $\beta=\frac{1}{2}$,

$$1-e^{-a^2}\cos 2x+e^{-4a^2}\cos 4x-e^{-9a^2}\cos 6x+\&c.$$
$$=\frac{1}{\sqrt{\pi}}e^{-\frac{1}{4}a^2}\int_0^\infty e^{-t^2}\left\{\frac{\cos at+e^{-a^2t}\cos(3at+2x)}{1+2e^{-a^2t}\cos(2at+2x)+e^{-4a^2t}}\right.$$
$$\left.+\frac{\cos at+e^{-a^2t}\cos(3at-2x)}{1+2e^{-a^2t}\cos(2at-2x)+e^{-4a^2t}}\right\}dt.$$

§ 5. In virtue of the formula

$$\frac{\sqrt{\pi}}{a}\left\{1+2e^{-\frac{\pi^2}{a^2}}\cos\frac{2\pi x}{a}+2e^{-\frac{4\pi^2}{a^2}}\cos\frac{4\pi x}{a}+2e^{-\frac{9\pi^2}{a^2}}\cos\frac{6\pi x}{a}+\&c.\right\}$$
$$=e^{-x^2}+e^{-(x-a)^2}+e^{-(x+a)^2}+e^{-(x-2a)^2}+e^{-(x+2a)^2}+\&c. \quad\ldots\ldots(3),$$

it is clear that a summation of the series

$$e^{-a^2}+e^{-(a-b)^2}+e^{-(a+b)^2}+\&c.$$

gives rise to an expression for the theta function.

By the use of Kummer's method, it will be found that, if $a$ and $b$ have the same sign,

$$e^{-a^2}+e^{-(a-b)^2}+e^{-(a-2b)^2}+\&c.$$
$$=\frac{2}{\sqrt{\pi}}e^{(r^2-1)a^2}\int_0^\infty e^{-t^2}\frac{\cos 2rat-e^{-2(r-1)ab}\cos(2ra+2b)t}{1-2e^{-2(r-1)ab}\cos 2bt+e^{-4(r-1)ab}}dt,$$

$$e^{-a^2}+e^{-(a+b)^2}+e^{-(a+2b)^2}+\&c.$$
$$=\frac{2}{\sqrt{\pi}}e^{(r^2-1)a^2}\int_0^\infty e^{-t^2}\frac{\cos 2rat-e^{-2(r+1)ab}\cos(2ra+2b)t}{1-2e^{-2(r+1)ab}\cos 2bt+e^{-4(r+1)ab}}dt,$$

$r$ being arbitrary subject only to the condition that it must be greater than unity in the first formula, and greater than zero in the second. Putting $r = 2$, we thus obtain for the theta function the expression[1]

$$\Theta(x+K) = \frac{K}{\pi} e^{\frac{3}{4}\frac{\pi x^2}{KK'}} \int_0^\infty e^{-\frac{KK'}{4\pi}t^2} \left\{ -\cos xt \right.$$

$$+ \frac{\cos xt - e^{-\frac{\pi x}{K'}} \cos(x+K)t}{1 - 2e^{-\frac{\pi x}{K'}} \cos Kt + e^{-\frac{2\pi x}{K'}}}$$

$$\left. + \frac{\cos xt - e^{-\frac{3\pi x}{K'}} \cos(x+K)t}{1 - 2e^{-\frac{3\pi x}{K'}} \cos Kt + e^{-\frac{6\pi x}{K'}}} \right\} dt.$$

§ 6. A third method of obtaining the value of $\Theta(x)$ is by means of the integral

$$\int_0^\infty \frac{\cos ct}{a^2+t^2}\, dt = \tfrac{1}{2}\frac{\pi}{a} e^{-ac} \quad\text{.....................}(4),$$

we thus have

$$e^{-a^2} + e^{-(a-b)^2} + e^{-(a+b)^2} + e^{-(a-2b)^2} + e^{-(a+2b)^2} + \&c.$$

$$= \frac{1}{\pi}\int_0^\infty \left( \frac{2a^2}{a^4+t^2} + \frac{2(a-b)^2}{(a-b)^4+t^2} + \frac{2(a+b)^2}{(a+b)^4+t^2} + \&c. \right) \cos t\, dt,$$

and it is found that

$$\frac{2a^2}{x^4+a^4} + \frac{2(a-\pi)^2}{x^4+(a-\pi)^4} + \frac{2(a+\pi)^2}{x^4+(a+\pi)^4} + \&c.$$

$$= \frac{1}{x\sqrt{2}} \left\{ \phi(x,a) + \phi(x,-a) \right\},$$

where
$$\phi(x,a) = \frac{\sinh x\sqrt{2} - \sin(x\sqrt{2}+2a)}{\cosh x\sqrt{2} - \cos(x\sqrt{2}+2a)},$$

leading to the formula

$$\Theta(x) = \sqrt{\frac{2}{\pi}} \cdot \int_0^\infty \{\phi(\alpha t, u) + \phi(\alpha t, -u)\} \cos(t^2)\, dt,$$

[1] The three formulæ in this section were given without proof in the *Messenger of Mathematics*, t. v. p. 173 (March 1876): but there are several misprints, and in the expression for $\Theta x$, the $x$ that occurs in the exponentials should be replaced by $x - K$.

$\phi$ being as just defined, and

$$\alpha = \frac{\pi K'}{K}, \quad u = \frac{\pi}{2K}(x + K).$$

Similarly, using the integral

$$\int_0^\infty \frac{t \sin ct}{a^2 + t^2} dt = \tfrac{1}{2} \pi e^{-ac} \dots\dots\dots\dots\dots\dots (5),$$

we find

$$\frac{2x^2}{x^4 + a^4} + \frac{2x^2}{x^4 + (a - \pi)^4} + \frac{2x^2}{x^4 + (a + \pi)^4} + \&c.$$

$$= \frac{1}{x\sqrt{2}} \{ f(x, a) + f(x, -a) \},$$

and $$\Theta(x) = \sqrt{\frac{2}{\pi}} \cdot \int_0^\infty \{ f(\alpha t, u) + f(\alpha t, -u) \} \sin(t^2)\, dt$$

where $$f(x, a) = \frac{\sinh x\sqrt{2} + \sin(x\sqrt{2} + 2a)}{\cosh x\sqrt{2} - \cos(x\sqrt{2} + 2a)},$$

and $\alpha$, $u$ are as before. These are the most simple and interesting of the expressions for $\Theta(x)$ as definite integrals.

The paper also contains the value of the series

$$e^{-a^n} + e^{-(a-b)^n} + e^{-(a+b)^n} + \&c.$$

as a definite integral, obtained by means of (4) and (5). The results are stated in the British Association Report, Glasgow, 1876, Transactions of Sections, pp. 15, 16. The integrals (4) and (5) were used to sum the series $1 \pm q + q^4 \pm q^9 + \&c.$ in my paper, *On the summation by definite integrals of geometrical series of the second and higher orders*, (Quart. Math. Journ. t. xi. p. 328—343, 1871), but I did not there obtain the value of $\Theta(x)$.

By putting $x = 0$, and $= -K$ in these formulæ, we find

$$1 - 2q + 2q^4 + 2q^9 - \&c. = \sqrt{\frac{2k'K}{\pi}}$$

$$= 2\sqrt{\frac{2}{\pi}} \cdot \int_0^\infty \frac{\sinh \beta t + \sin \beta t}{\cosh \beta t + \cos \beta t} \cos(t^2)\, dt$$

$$= 2\sqrt{\frac{2}{\pi}} \cdot \int_0^\infty \frac{\sinh \beta t - \sin \beta t}{\cosh \beta t + \cos \beta t} \sin(t^2)\, dt,$$

and

$$1 + 2q + 2q^4 + 2q^9 + \&c. = \sqrt{\frac{2K}{\pi}}$$

$$= 2\sqrt{\frac{2}{\pi}}\int_0^\infty \frac{\sinh \beta t - \sin \beta t}{\cosh \beta t - \cos \beta t}\cos (t^2) dt$$

$$= 2\sqrt{\frac{2}{\pi}}\int_0^\infty \frac{\sinh \beta t + \sin \beta t}{\cosh \beta t - \cos \beta t}\sin (t^2)\, dt,$$

where
$$\beta = \sqrt{2}\,\frac{\pi K'}{K} = \sqrt{2}\log\left(\frac{1}{q}\right).$$

These results may be compared with those given in the *Quarterly Journal.* The latter were however obtained by the direct method explained in the next section, and the formulæ are not identical.

§ 7. We may also apply the integrals (4) and (5) to the direct summation of the series in (1), and it is thus found that

$$-1 + \Theta(x) = \frac{4K}{\pi}\int_0^\infty \cos\left(\frac{2KK'}{\pi}t^2\right)\frac{M}{\cosh 2Kt - \cos 2Kt}dt$$

$$\Theta(x) = \frac{4K}{\pi}\int_0^\infty \sin\left(\frac{2KK'}{\pi}t^2\right)\frac{N}{\cosh 2Kt - \cos 2Kt}dt,$$

where

$$M = \sinh(K-x)t\cos(K+x)t + \sinh(K+x)t\cos(K-x)t$$
$$- \cosh(K-x)t\sin(K+x)t - \cosh(K-x)t\sin(K+x)t,$$

and $N$ = a like expression, the only difference being that the signs of all four terms are positive. Here $x$ is supposed to lie between $K$ and $-K$.

§ 8. It will be noticed that none of the expressions for $\Theta(x)$ put in evidence any of the properties of the function, and it does not appear that it would be easy to deduce any theorems from the integral expressions: but it is interesting to compare the different integrals obtained by the four distinct methods.

I should mention that as nearly all the results stated in this abstract were obtained by rather lengthy analytical processes, there is a possibility of error. All the work was throughout performed twice independently, but at a very short interval of time, and before the memoir is finally presented to the Society I intend to recalculate *de novo* all the expressions.

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A communication was also presented, by Mr J. W. WARREN, *On Curvilinear and Normal Coordinates* (4th Exercise). This will appear in the *Transactions* of the Society, and does not admit of being given in abstract with advantage.

*May* 21, 1877.

Prof. Clerk Maxwell, F.R.S., President, in the chair.

A communication was made to the Society by

(1) Prof. T. McKenny Hughes, *On the base of the Silurian system;* with note by Mr Marr, *On the Phacopidae of the Lake District.*

Professor Hughes read a paper, in which he attempted to fix more exactly the base of the Silurian (Sedgwick) system in the north of England, and traced the bearing of the results there obtained upon the Welsh sections. At Austwick near Settle the Basement Bed was a conglomerate resting on Upper Bala Beds and covered by the Coniston Flags. At Crag Hill in the upper part of Ribblesdale it was a brecciated limestone resting immediately on the Coniston Limestone and succeeded by the Coniston Flags. In neither of these cases had he detected any traces of the Graptolithic Mudstones. Further north in the Sedbergh district he found in several sections a similar conglomerate succeeded by shales which passed up into the Graptolithic Mudstone, which, through the Pale Slates, passed up into the Coniston Flags. Still further north in Spengill, a tributary of the Rothey, he found the Coniston Flags passing down through Pale Slates into the Graptolithic Mudstone, in the upper part of which was a thin limestone with fossils, to which he gave the name of Spengill Limestone. The Graptolithic Mudstone passed down into shale, at the base of which (just where the conglomerate appears in the Sedbergh district) was a bed of calcareous sandstone with fossils, which he thought was probably the Basement Bed.

Crossing over to the Lake District, in Skelgill, behind the Low Wood Hotel, Windermere, the Coniston Flags passed down through the Pale Slates into the Graptolithic Mudstone, at the base of which was a pale calcareous sandy mudstone which he took as the Basement Bed, and which rested on other pale slates. These last passed down into, and he thought belonged to, the Upper Bala series. Further west, in Ash Gill near Coniston, the Coniston Flags pass down through pale slates into the Graptolithic Mudstone, which in turn passes down into a knobbly pyritous band representing the Basement Bed. This rests on the Ash Gill slates, which pass down into, and seem to belong to the Coniston Limestone series.

The Basement Bed is thus either a calcareous conglomerate, as at Austwick and near Sedbergh, or a limestone sometimes with a brecciated structure, as at Crag Hill, or a calcareous sandstone or mudstone as at Spengill and Skelgill, or a concretionary bed in which evidently much chemical change has been going on, as at Ash Gill. It is almost always full of pyrites. He paralleled the Coniston Flags and Grits with the Denbigh Grits and Flags; the Pale Slates with the Tarannon Shale and Pale Slates of Wales; the Graptolithic Mudstones with the banded beds of Aberhirnant, &c., and the Basement Bed with the Corwen Grit and conglomerate and with the Limestone of Llansantfraidglynceiriog. He gave lists of fossils, pointing out that, as far as his observation had gone, Cardiola interrupta never occurred below the Coniston Flags, nor Trinucleus above the Bala Beds; while Meristella crassa was as yet peculiar to the Basement Bed. It was found at Spengill; in the Corwen Grits of Cyrnybrain: in the Llandovery Rocks at Llandovery; and, according to the survey, in the equivalent beds in other districts.

*Note on the Phacopidae of the Lake District*, by J. E. Marr, S. John's Coll.

The author considered the Phacopidae peculiarly adapted to shew the palæontological differences between the Cambrian and Silurian of the Lake District; 1st, on account of their occurrence all through the series; and, 2ndly, on account of the limitation of each species, so far as yet known, to small thicknesses of strata.

He gave the following list of the more important species, with the beds which they characterise:

| | | Zone of |
|---|---|---|
| Silurian .. | Kirkby Moor Flags. ....... | |
| | Bannisdale Slates.......... | Phacops (Acaste) Downingiae. |
| | Coniston Grits.............. | |
| | Coniston Flags ............ | Phacops (Odontocheile) obtusicaudatus. |
| | Pale Slates .................. | |
| | Graptolithic Mudstones .. | Phacops (Phacops) Musheni? |
| | Basement bed of Silurian | |
| Cambrian | Upper Bala Shales......... | Phacops (Odontocheile) mucronatus var.<br>Phacops (Acaste) apiculatus. |
| | Coniston Limestone ...... | Phacops (Chasmops) conophthalmus.<br>Phacops (Chasmops) macroura. |
| | Green Slates ............... | (No fossils). |
| | Skiddaw Slates............. | Phacops (Acaste) Nicholsoni. |

Referring to the above list he observed that the two forms of Phacops most common in the Coniston Limestone belonged to the subgenus Chasmops, a Cambrian subgenus; that the two species found in the overlying Upper Bala Shales were of great interest,

as one or both occurred, wherever these beds had been found, and had not yet been discovered in the Coniston Limestone proper; the two Chasmops again having never been found in the Upper Bala beds. He observed that the conclusion pointed to by the Phacopidae was fully borne out by the other fossils of these two formations, as also by lithological characters, viz., that the Upper Bala Shales were altogether newer than the Coniston Limestone, and were not merely due to local conditions, and passing elsewhere into limestones; and hence he concluded, that as the shales varied greatly in thickness, in passing from place to place, and were sometimes altogether absent, that they were either very irregularly deposited, or else subsequently denuded, in either case pointing to a break before the deposition of the next overlying beds.

The author also remarked on a Phacops in the graptolithic mudstones, as being of the subgenus Phacops proper, and probably referable to Ph. Musheni, an exceedingly characteristic Silurian form.

In treating of the Coldwell beds, he observed that these beds seem to have been sometimes confused with the Upper Bala series, being very like them in lithological character, and containing a species of Phacops, of the subgenus Odontocheile, exceedingly like the one [P. (O.) mucronatus var.] in the lower series, but differing in well-marked characters. The Coldwell beds occur across the Troutbeck Valley, on the east side of Windermere, as well as at the typical place on the west side of the Lake, and are high up in the Coniston Flag series. He concluded that the P. mucronatus var. of the lower series had migrated on change of conditions after the end of the Upper Bala period, and had returned to the original area on the recurrence of conditions similar to those of its former habitat, having changed by variation to P. obtusicaudatus in the meantime.

In conclusion, he referred to the mention of Orthis, apparently O. vespertilio, having been found by Prof. Nicholson in these Coldwell beds (Nich. *Essay on Geol. of Cumberland and Westmorland*, p. 61), which were undoubtedly Silurian, considerably diminishing the value of this fossil as a test of the Cambrian nature of beds containing it in this district, and that hence much stress cannot be laid on its occurrence in the graptolithic mudstones, as stated by Profs. Harkness and Nicholson in a recent paper at the Geological Society (*Q. J. G. S.* Vol. XXXIII. p. 473).

(2) Mr PEARSON, *On some points in the history of Astronomy.*

Mr PEARSON read a paper on some passages from the classics, one from Hesiod, and three from Ovid, which he considered might be fairly tested by Modern Astronomy. Admitting, as is often averred, that many allusions of this nature in the classics are either inaccurate or wrong, some he thought might still be found to have the stamp of truth about them.

On the one hand, it is certain that in Greece the phenomena of the heavens had from the earliest times many thoughtful and attentive observers. In the time of Hesiod, which may be perhaps best assumed to have been about the middle of the eighth century B.C., the rising and setting of the stars seems to have been the recognised guide in distinguishing the successive seasons of the year: the Metonic cycle, now known under the title of the *Golden Numbers,* was discovered in the time of Socrates: and the ordinary authorities, such as the article *Astronomia* in the Dictionary of Antiquities, show how much interest the subject attracted down to the period of Ptolemy and Hipparchus. On the other hand, it must be allowed that the references we can actually find in classical authors are often vague or rhetorical; and that, probably excepting Hesiod, those whose writings we refer to wrote on second-hand authority. It may be therefore fully admitted that the question requires to be investigated with much caution.

The first reference was to Hesiod (*Op. et Di.* 564—7), as being the most distinct passage in that author's writing, although there are others which deserve consideration as *data* in Practical Astronomy: these lines, Mr Pearson said, he thought deserved the best attention, as the whole character of the work in which they occur is most genuine and natural, nor is it easy to study it without the impression that the author was himself dependent, as a practical agriculturist, on the facts that he recites.

The passage itself runs thus:

> *Εὖτ' ἂν δ' ἑξήκοντα μετὰ τροπὰς ἠελίοιο*
> *Χειμέρι' ἐκτελέσῃ Ζεὺς ἤματα, δὴ ῥα τότ' ἀστὴρ*
> *Ἀρκτοῦρος, προλιπὼν ἱερὸν ῥόον Ὠκεανοῖο*
> *Πρῶτον παμφαίνων ἐπιτέλλεται ἀκροκνέφαιος.*

From this we learn that, sixty days after the winter solstice, Arcturus rose during twilight in the evening. Arcturus' position for Jan. 1, 1875, is given in the *Nautical Almanac* as R.A. 14 h. 9 m. 55 s., Dec. 19° 50′ $22\frac{1}{2}''$ N. If we convert these *data* into Latitude and Longitude, reduce the star's longitude by about 36° 10′, which at the annual rate of 50″·1 for *precession* will bring us to about 730 B.C., and reconvert the star's new *longitude* and *latitude* into R.A. and Dec., we shall find that the position of the

star in the early part of the eighth century B.C., which may be fairly taken to represent the era of Hesiod, was something about 12 h. 6 m. R.A. and 33° 30′ North Dec. On Feb. 20 at that time, in Lat. $38\frac{1}{2}°$ N., about the situation of Ascra and Helicon, the Sun would set about 5.40 p.m., while Arcturus would rise above the horizon about 5.53 p.m., a relative position of the two luminaries which fairly answers to the words of the poet. And while investigating the position of the star, Mr Pearson said he found he had unintentionally explained, as he believed, the epithet "late-setting," applied to Arcturus in Hom. *Od.* E′ 272. Arcturus at that epoch would first have been visible at the time of its morning setting about May 24, and would set June 1 at 3.30 a.m., July 1 at 1.32 a.m., Aug. 1 at 11.30 p.m. During the early summer therefore, when the Greek seaman or agriculturist was often spending the nights out of doors, the late time at which this brilliant star would set must have been quite unmistakeable, and Ulysses is naturally described as keeping his eye fixed on it, as carefully as he kept the Bear on his left, to determine his voyage eastwards.

In order to satisfy criticism, the series of computations by which this result is obtained are given: the computations will be omitted in two of the subsequent examples, but any one who will employ the same formulæ will find that the results given are approximately accurate. It is probable that theoretical astronomers may be able to suggest better or more precise methods of obtaining the required results, but those employed have the advantage of being quite simple, and are anyhow approximately correct. The calculation of Arcturus' place for the era of Ovid is also given, as it naturally accompanies that for the time of Hesiod.

The formulæ employed are those given in *Loomis's Astronomy*, and are the following:

(1) To reduce R.A. and Dec. to Long. ($L$) and Lat. ($l$).

Let $A$ be a subsidiary angle: $\omega$ the inclination of the ecliptic,

$$\tan A = \sin \text{R.A.} \,.\, \cot \text{Dec.},$$

$$\tan L = \sin (A + \omega) \tan \text{R.A.} \operatorname{cosec} A,$$

$$\tan l \;\; = \sin L \cot (A + \omega).$$

(2) To perform the reverse process:

$L'$ being the new Long. due to change from precession, $A'$ the subsiding angle,

$$\tan A' \quad = \sin L' \cot l,$$

$$\tan \text{R.A.} = \sin (A' - \omega) \tan L' \operatorname{cosec} A',$$

$$\tan \text{Dec.} = \sin \text{R.A.} \cot (A' - \omega).$$

We apply these formulæ to find the place of Arcturus about the era of Hesiod.

Taking the mean position of the star as given above: then

$$\begin{aligned} \sin \text{R.A.} &= \phantom{1}9{\cdot}7299685\,(-) \\ \cot \text{Dec.} &= \underline{10{\cdot}4427302\,(+)}. \\ & \phantom{=} 10{\cdot}1726987.(-) = \tan 303^\circ\, 53'\, 49'' = \tan A, \end{aligned}$$

and $(A+\omega)$ consequently $= 327^\circ\, 23'\, 49''$, making $\omega$ somewhat freely $= 23\frac{1}{2}^\circ$.

Again, we have

$$\begin{aligned} \sin (A+\omega) &= \phantom{1}9{\cdot}7314403\,(-) \\ \tan \text{R.A.} &= \phantom{1}9{\cdot}8038388\,(+) \\ \operatorname{cosec} A &= \underline{10{\cdot}0808999\,(-)} \\ & \phantom{=1} 9{\cdot}6161790\,(+) = \tan 202^\circ\, 27'\, 5'' = \tan L. \end{aligned}$$

Also

$$\begin{aligned} \sin L &= \phantom{1}9{\cdot}5819490\,(-) \\ \cot (A+\omega) &= \underline{10{\cdot}1940903\,(-)} \\ & \phantom{=1} 9{\cdot}7760393\,(+) = \tan 30^\circ\, 50'\, 28'' = \tan l. \end{aligned}$$

The next step is, taking the amount of annual precession, it is owned somewhat boldly, at $50''{\cdot}1$, to estimate its amount first for 1900 years to bring it to 27 B.C., about the era of Ovid, and again for 700 years, to bring it to that of Hesiod. The first amount is about $26^\circ\, 26'\, 30''$, and the second about $9^\circ\, 44'\, 30''$, which will bring us to $176^\circ\, 0'\, 35''$ as the Long. in the time of Ovid, and $166^\circ\, 16'\, 5''$ in that of Hesiod. As it is certain that the inclination of the ecliptic has not changed more than $20'$ to $30'$, within the periods in question, we may safely deal with the Latitude of the star as stationary in the interval. Consequently, $L$, $L'$ being the Longitude of the star, in the time of Hesiod and of Ovid: $l$ its latitude in both: $L = 166^\circ\, 16'\, 5''$, $L' = 176^\circ\, 0'\, 35''$, $l = 30^\circ\, 50'\, 28''$, and on these data we proceed to compute its R.A. and Dec., and from these the times of the star's rising and setting at these two epochs.

| | | | |
|---|---|---|---|
| $\sin L$ ........ | $9{\cdot}3754437\,(+)$ | $\sin L'$ ..... | $8{\cdot}8422274\,(+)$ |
| $\cot l$ ........ | $10{\cdot}2239607\,(+)$ | $\cot l$ ........ | $10{\cdot}2239607\,(+)$ |
| $\tan \alpha$ ........ | $9{\cdot}5994044\,(+)$ | $\tan \alpha'$ ..... | $9{\cdot}0661881\,(+)$ |
| $\alpha$ ........ | $201^\circ\, 40'\, 51''$ | $\alpha'$ ........ | $186^\circ\, 38'\, 34\frac{1}{2}''$ |
| $\omega$ ..... | 23 50 0 | $\omega$ ........ | 23 45 0 |
| $(\alpha-\omega)$ ... | 177 50 51 | $(\alpha'-\omega)$... | 162 53 $34\frac{1}{2}$ |
| $\sin(\alpha-\omega)$... | $8{\cdot}5747184\,(+)$ | $\sin(\alpha'-\omega)$... | $9{\cdot}4685814\,(+)$ |
| $\tan L$ ..... | $9{\cdot}3880381\,(-)$ | $\tan L'$ ..... | $8{\cdot}8435834\,(-)$ |
| $\operatorname{cosec} \alpha$ ..... | $10{\cdot}4324609\,(-)$ | $\operatorname{cosec} \alpha'$ ..... | $10{\cdot}9367372\,(-)$ |
| $\tan$ R.A. ... | $8{\cdot}3952174\,(+)$ | $\tan$ R.A. ... | $9{\cdot}2489020\,(+)$ |

∴ R.A. = 12 h. 5 m. 42 s. R.A. 12 h. 40 m. 14 s.

| | | | |
|---|---|---|---|
| sin R.A....... | 8·3956475 (−) | sin R.A. ... | 9·2421704 (−) |
| cot $(\alpha-\omega)$... | 11·4249751 (−) | cot $(\alpha-\omega)$ | 10·5117660 (−) |
| tan N. Dec. | 9·8206226 (+) | tan N. Dec. | 9·7539364 (+) |
| N. Dec.... | 33° 29' 25" | N. Dec. = | 29° 34' 24". |

We thus ascertain the position of the star in the time of Hesiod and in that of Ovid, to have been: for that of Hesiod, R.A. 12 h. 5 m. 42 s.... N. Dec. 33° 29' 25"; for that of Ovid, R.A. 12 h. 40 m. 14 s. N. Dec. 29° 34' 24".

The next step is to compute the hour angle of the star at its true rising, first for the Latitude of Bœotia, about 38½ N., secondly for the Latitude of Rome, about 42° N., and also the Local mean time at the same moment.

In Bœotia

| | | | | |
|---|---|---|---|---|
| cos H.A. = − tan Dec. tan lat. | | | | |
| tan Dec. = | 9·8206228 | | | |
| tan lat. = | 9·9006052 | | | |
| cos H.A. | 9·721228. (−) | | | |
| | h | m | s | |
| = | 8 | 7 | 1 | |
| R.A. ......... | 12 | 5 | 42 | |
| | 3 | 58 | 41 | |
| S.T. Feb. 19. | 22 | 0 | 0 | |
| Time of Arcturus rising... | 5 | 58 | 40 | p.m. |

Local mean time of Sunset on the same day. 5 39 0 p.m.

As soon therefore as the daylight had sufficiently diminished for the star to be visible, it would actually rise.

At Rome

| | | | |
|---|---|---|---|
| cos H.A. = − tan Dec. tan lat. | | | |
| tan Dec... | 9·7539380 | | |
| tan lat. ... | 9·9544374 | | |
| cos H.A. | 9·7083754 (−) | | |
| | h | m | s |
| H.A. ...... = | 8 | 2 | 54 |
| | 12 | 40 | 14 |
| | 20 | 43 | 8 |
| S.T. May 26. | 4 | 15 | 8 |
| Time of Arcturus setting for May 26. | 16 | 28 | 0 |
| S.T. June 6. | 4 | 58 | 8 |
| Time of Arcturus setting for June 6. | 15 | 45 | 0 |

The Sun rises at Rome on May 26 about 4.35 a.m., on June 6 about 4.30 a.m. According to Ovid, the star's morning setting was first visible on May 26, or, as he states later on, on June 6. If we consider him to have consulted two different authorities, one of which gave the true, the other the visible setting of the star, no reasonable exception can be taken to the value of his statements. The expressions the poet uses point to the time when the star's setting first occurred before sunrise; this for theoretical astronomers would actually have taken place about May 26, and for practical observers about June 6, the star setting on the first-named day at 4.28 a.m., on the second at 3.45 a.m.

Again in the Fasti of Ovid, I. 654, II. 76, we are told that

Lyra, or *Vega*, was last visible when setting in the evening, about Feb. 1. "Ubi est hodie, quae Lyra fulsit heri?" Employing again the method of calculation indicated above, we find on that day at Rome the Sun would set about 5.10 p.m., and Lyra about 5.44. As the days at that time of the year are rapidly lengthening, while the star would set earlier every day, it is obvious that the date assigned for the last appearance of the latter is nearly exact.

He makes however a remark about Capella which seems really erroneous. He says (Fasti v. 113) that she rises on May 1st, *i.e.* is then first visible in the morning. But at the time when Ovid lived she would, according to the mode of computation used in the previous examples, have risen about 3.0 a.m., while the Sun would not have risen until after 5.0. We have a similar apparent mistake in Pliny and Columella, nearly contemporaries, who flourished in the latter half of the first century A.D. They fix Arcturus' rising for the 23rd or 21st of February; whereas on those days the Sun would set at Rome about 5.35 p.m., while the star would not pass the horizon in their time before 6.30 p.m. They seem to have copied from Hesiod without any thought[1].

The late Mr F. Baily, in his edition of *Ancient Star Catalogues*, published in Vol. XIII. of the *Memoirs of the Royal Ast. Society*, does not seem to have actually compared the positions there given to any of the principal stars with those which in the present day we must suppose them to have then occupied, though he refers to Delambre (*Hist. Ast. Anc.* Vol. II.), who gives tabulated results on this point from his own calculations. As however the present rate of change in the obliquity of the ecliptic would have made it in the time of Eratosthenes (230 B.C.) about 23° 43′, whereas that astronomer fixed it roughly at 23° 51′, it is to be hoped that, making allowance for inaccuracies in the MSS., such a process of verification may be attempted with some prospect of success; and possibly some explanation found of Ptolemy's idea, that in his time (A.D. 140) the amount of annual *precession* was only 36″. It is curious that the error of 15′ in the latitude of Alexandria, which Delambre imputes to the Greeks, answers nearly exactly to the obliquity of 23° 43′, to which we are brought by its present known rate of change.

Mr Glaisher also communicated to the Society a sketch of his *Table of Exponential Functions*, an abstract of which cannot be conveniently given, but which will probably appear in the next part of the Society's *Transactions*.

[1] In the time of Ovid the position of *Vega* must have been about R.A. 17h. 29m., Dec. 38° 23′ N.; that of *Capella*, R.A. 2h. 55m., Dec. 40° 35′ N.

*October* 22, 1877.

PROF. CLERK MAXWELL, F.R.S., PRESIDENT, IN THE CHAIR.

A communication was made to the Society by

MR BALFOUR, *On the Development of the Vertebrate Ovum.*

The points dealt with in this paper were (1) the nature of the stroma of the ovary, and (2) the relation of the permanent ova to the large cells of the germinal epithelium, named primitive ova by Waldeyer.

With reference to the first point the author shewed the stroma of the ovarian part of germinal ridge to be in Elasmobranchs a derivative of the germinal epithelium, and not, as has usually been supposed, of the subjacent mesoblast, and pointed out the bearings of his conclusions on the controversies which have arisen as to the nature of the follicular epithelium.

In dealing with the second point he stated that the permanent ova were not direct derivates from the primitive ova, but that by a rapid division of the nuclei of the latter polynuclear masses were formed, comparable with the egg-tubes of many Invertebrata. From these masses the permanent ova took their rise by the same series of steps as in the egg-tubes of the Invertebrata.

*October* 29, 1877.

PROF. BABINGTON, VICE-PRESIDENT, IN THE CHAIR.

A communication was made to the Society by

Prof. LIVEING, *On the metamorphism of the rocks of the Channel Islands.*

IN the Autumn of 1876 I spent three days in Guernsey in company with Mr OSMOND FISHER, and was much struck by the variation in the amount of metamorphism which the rocks there appeared to have undergone. I have this summer again visited the island, and have also visited the other islands of the group, but my remarks relate principally to Guernsey.

The rocks of Guernsey consist of two principal divisions. The upper one is a sort of gneiss, consisting for the most part of layers of imperfectly crystallized red felspar, (which has been erroneously described as scapolite) inter-stratified with layers of quartz, and sometimes hornblende and sometimes mica. This forms the part of the island south of a line from St Peter Port to Vazon Bay. To the north of this line on the west side of the island the gneiss passes rather abruptly into a pink-coloured, highly crystalline syenite, consisting of felspar, quartz and hornblende, with here and there a very little mica, shewing no trace of stratification, used for building in the island but not quarried for export. On the east side the gneiss passes, also somewhat abruptly, into a dark grey syenite, consisting of hardly anything but hornblende and felspar, sometimes shewing traces of stratification but in other parts wholly devoid of any such traces. This grey syenite is largely exported for paving the streets and macadamizing the roads about London. The parts which have the crystals of hornblende disposed in strata (often hardly noticeable in hand specimens) are split with tolerable regularity and wrought for curb and pitching stones, while the other parts where all traces of stratification have disappeared are broken up for macadam metal. There are some places (e.g. in quarries in low ground between Delancy Hill and Noirmont) where hornblende predominates and where the grain is so fine and uniform that the stone works pretty freely in all directions, and they break it into pitching stones just as it comes.

Ansted, in his work on the Channel Islands, states that these two divisions, the syenite on the north and the gneiss on the south, are parts of one metamorphic system. He does not trace the evidence by which he arrived at this conclusion, and at first sight it was rather difficult to believe that the highly crystalline syenites and hornblende rocks were parts of the stratified system seen in the south of the island. A closer examination convinced me that his statement is correct. In some places the gneiss may be traced passing gradually into grey syenite. This is seen on the south side of Vazon Bay near Richmont barracks: and further south near Fort Grey there is a quarry in a very tough hornblendic gneiss which seems to be the transition condition of the rock. The junction of the red gneiss with the grey syenite is obscured on the east side by the town and harbour, and inland by the surface soil. However, on the shore of the small bay under Fort George there is grey syenite apparently interstratified with the gneiss. They form there a perfectly continuous formation. Patches of grey hornblendic gneiss are seen in the island of red gneiss on which Castle Cornet stands. All question as to the syenite being a stratified rock metamorphosed is set at rest by the occurrence of a bed of stratified quartzite in it. This occurs in a

quarry belonging to Mr Mowlem, on the west side of Delancy Hill near St Sampson's. It is about 5 feet thick where it is exposed, evidently stratified, and dipping towards the north-west. It thins out on the east side, but appears to increase in thickness to the west. The quarry has been wrought down to it through some 30 feet of grey syenite, and the quarrymen told me they had tested the thickness of it, and finding that it increased in thickness to the west they did not attempt to remove it. They are now working on the south-east side and underneath it, merely removing it as it falls.

The chief mineralogical difference between the upper and the lower division of the rocks is that the upper contains a good deal of quartz and some mica, while the lower contains scarcely any quartz and no mica, but a great deal of hornblende.

In the neighbouring island of Sark, only about 7 miles distant[1], the lower hornblendic beds have not undergone nearly so complete a crystallization. The greater part of Sark consists of a hornblende schist, overlaid at the north-west by a gneiss not unlike that of Guernsey, but less red in colour. I have no doubt that these are really the same beds in both islands. In the south of Sark there is a grey syenite closely resembling some of the Guernsey syenite.

There are plenty of volcanic dykes which produce some of the characteristic features of the coast scenery, but they mostly cut clean through the metamorphic rocks and seem to have had little or no effect upon the parts in contact with them. The greater part of these dykes are of a fine grained compact greenstone, not more than a few feet thick, and they are disintegrated by the sea and weather much more rapidly than the metamorphic rocks. The promontory on which Richmont barracks stand between Perelle and Vazon Bays is cut through by some of these greenstone dykes. The gneiss there is highly hornblendic. On the Vazon Bay side it retains its stratified character, while on the Perelle Bay side it forms a highly crystalline syenite shewing no trace of stratification. It may be questioned whether the dykes have anything to do with these characters. I think not, because they extend to much too great a distance from the dykes, and because in other places no such effects are produced by the dykes.

There is another set of dykes of a different material, consisting of a very compact reddish felspathic matrix with small lumps of quartz imbedded in it. These dykes are seen at the bathing-place by St Peter Port, where the road is cut through them, and they run on through the island on which Castle Cornet stands. These red dykes are not parallel to the greenstones ones, and must have

[1] The actual distance on the shortest line as measured on the Admiralty Chart is a little less than $6\frac{3}{4}$ miles.

been formed at a time when the direction of the pressure below was such as to open a different line of joints. I believe they are of much older date than the greenstone dykes, and that they have undergone a certain amount of metamorphosis whereby the porphyritic character has been imparted to them.

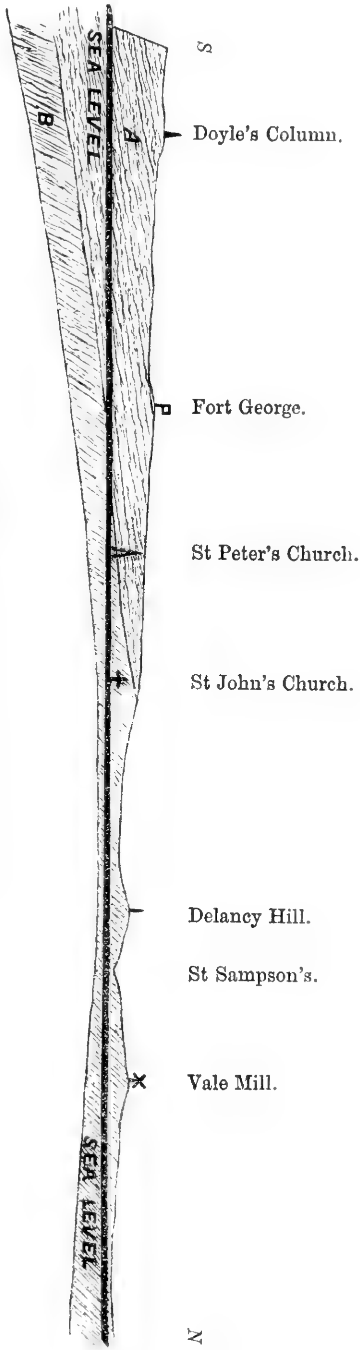
The gneiss in contact with them appears at least in places to have become indurated, the stratification is not at all obliterated, but the rock has acquired more of a hornstone character, is less crystalline and perhaps contains a larger proportion of quartz; a change not produced by mere heating, as it is very unequally distributed.

One point to which I wish to draw particular attention is the extreme variation in the amount of metamorphism which the rocks have undergone within a very short distance.

The pink syenite of Cobo I thought at first must be intrusive, it is so highly crystalline and so entirely devoid of stratification; but it really seems to be only an altered state of the red gneiss. In the first place they quarry grey syenite, elsewhere proved to be stratified, at a very short distance, a few hundred yards north from the pink stone, and the quarrymen assured me in a very positive manner that none of the pink was to be found under the grey, however deeply they might go into it.

Moreover, in one quarry there are patches of grey in the pink stone, as if the original beds were there somewhat intermingled, as they are in the bay under Fort George and under Cornet Castle. On tracing the gneiss down the north side of the Vazon Bay Road it becomes very hard under the small square white-washed tower called the "Hougue," and gradually loses its stratified character as it passes westward. The stratification is obliterated without any material change in the mineralogical characters. The colour and texture of the rock into which it passes here are not the same as at Cobo, it is greyer and less coarsely crystallized; but the material of the Cobo rock is so nearly the same as that of a great deal of the gneiss, that apart from the complete obliteration of the stratification there is no difficulty in the identification of the two. In fact, the Cobo rock mineralogically belongs rather to the upper, more quartzose beds than to the lower hornblendic beds.

These lower beds are quarried all over the northern part of the island. The land there is generally low, but has a number of bosses of rock projecting above the general level. These are harder or tougher parts which have resisted the weather. Most of the quarries have been begun in these bosses, but it is often only when they have got well down, or well into the hill, that stone suitable for breaking into square blocks is found. The bosses are often full of veins and cross joints, and coarsely crystalline patches. These patches the workmen call "sunburnt," imagining their coarse tex-




DIAGRAMMATIC SECTION OF GUERNSEY FROM S. TO N.

*A.* Gneiss.

*B.* Syenite.

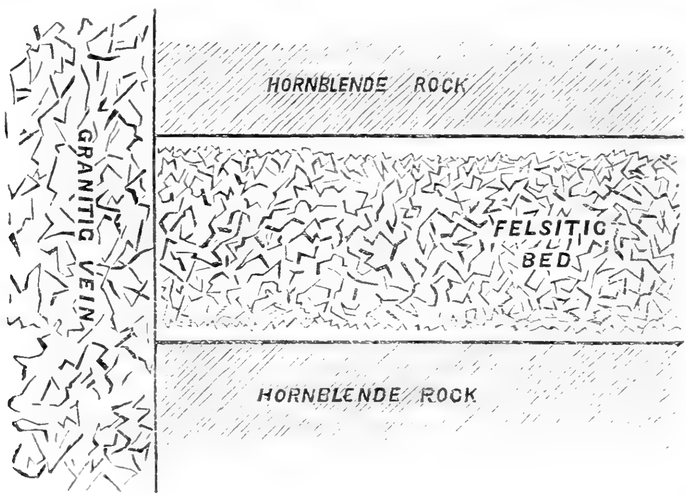
ture to be due to exposure. The veins are mostly felspathic, but in some cases contain various minerals, iron pyrites, galena, calcite and epidote. With one exception, which I shall mention hereafter, they are all small and plainly not intrusive, but connected with the more highly crystalline character of the rock in the places where they occur. This highly crystalline character shews itself not only in the obliteration of all stratification, but in the more complete separation of the minerals of which the rock is composed. The crystals become in many places much larger; a gradual separation of hornblende from felspar has gone on, each crystal seeming to collect material of its own kind, until a very coarse mixture is produced. This is a kind of result which we know to be produced artificially in mixtures of chemicals by slow partial solution and recrystallization, or partial decomposition and recomposition, under variations of temperature. In such processes the smaller crystals present the larger surfaces in proportion to their weight, and so become dissolved or decomposed in the greater proportion, while the crystallogenic law which causes the deposition of crystallizable material on crystals of its own kind determines the growth of larger and larger crystals, and thereby the more complete separation of the several mineral species. These veiny places are evidently places where the joints have been more open, and have allowed more free passage for air, water, steam and gases, agents which may have produced partial decomposition. Moreover it may be observed that some of the most highly crystalline parts are found near the junction of the north and south divisions of the island, as in Well Road quarry and other quarries near St John's Church in the town of St Peter Port, and at Cobo, at a very short distance from the gneiss, while the parts of the syenite which shew stratification are farther north and lie deeper, as at the very bottom of the deepest quarries near Vale Mill which are wrought below the sea level, so that the unstratified part lies between two stratified portions. There seems to have been an elevation, with its centre somewhat to the east of Guernsey, producing an anticlinal axis with a generally east and west direction somewhere about the line from the north of St Peter Port to Cobo, so that it is just in this part that the joints would be most opened by the process of elevation. It can hardly be supposed that these middle portions can have undergone fusion by heat, and I doubt whether the crystalline structure of granite is ever due to cooling from fusion in the sense generally understood, but is rather due to the metamorphic process before mentioned which will go on at comparatively low temperatures, but which necessitates, not a uniformly decreasing temperature, but one of continued variations. Variations of temperature will not in general be so frequent at a great depth below the surface as at moderate depths, so that

my observation of these rocks goes to shew that the granitic structure is due to a metamorphism going on at moderate depths and at temperatures far below the fusing point of the rocks. This is quite a different thing from the assertion that some syenites may not have been in a state of fusion at one time. This may well have been, but if so, the granitic structure was not left on their solidification but has been produced by subsequent metamorphosis.

The granitic veins in the islands throw some light on this. There are two kinds of granitic veins in Guernsey. First in the cliff under Fort George the gneiss is traversed by many branching veins. Some of these are ordinary quartz veins, but one at least is granitic or syenitic. It is a small vein very sharply defined, only about three inches thick, and the material filling it is a crystalline mixture of quartz, felspar, hornblende and mica. There is not the slightest reason to suppose it intrusive, in fact it is just like a common quartz vein, except as to material, and I have no doubt that it has been produced in a similar way. That is to say, a crack in the rock has been filled up with material derived from the rock itself, though in the case of this and other such granitic veins I think the material must have subsequently assumed the granitic crystallization. There are similar ramifying granitic veins in grey syenite at Croc du Hurté, the southern extremity of the Grève d'Azette near St Helier's, Jersey. In neither place is there any larger mass of the same structure with which they can be connected, although there are greenstone dykes close by. I have generally observed that the outside of a vein of this kind is composed almost entirely of felspar with the ends of the crystals pointing inwards. This probably has more to do with the process (of metamorphism) by which the crystallization was produced than with the mode in which the material got into the crack which it has filled up.

There is however another class of granitic veins, of which I found an admirable example in Guernsey, in a quarry in Delancy Hill adjoining the Grande Maison Road, near St Sampson's: in the same hill in which the stratified quartzite occurs, but on the opposite side of it, 300 or 400 yards off. The rock quarried here is a highly crystalline hornblende rock with but little felspar in its composition, but with various thin beds in it of a felsitic character, parallel to one another and to the general direction of the strata. These beds are some of them 12 inches thick, and others not more than $\frac{1}{4}$ inch, and in some places they are connected by thin cross veins which were probably joints which have been filled up. The material of these beds is in some places almost a fine grained quartzite, and in other places, though felspar predominates in its composition, it still contains a notable proportion

of fine grains of quartz. They have probably been sandy or loamy beds in the original deposit, and bear a close relation to the thicker quartzite deposit which appears on the other side of the same hill. Seen under a microscope the quartz grains in both deposits shew a similar rather peculiar corroded form. The hornblende syenite and the felsitic beds are both cut through by a large vein of granitic character, nearly vertical, thinning out at top and thickening down to the floor of the quarry, and perhaps further. It is seen on both sides of the quarry, which is a large one. A great deal of it has been removed, but where the quarrymen are now at work it is left standing. It is 5 or 6 feet thick where it is exposed and seems thickening as it descends. On the opposite side of the quarry it is much hidden by rubbish, but it rises there to a greater height and seems to divide into more than one vein. Mineralogically it consists of quartz, felspar, and a little hornblende with a few specks of mica. It is separated from the hornblende rock and the felsitic beds by a very thin layer of a sort of soapstone which I have no doubt is decomposed hornblende which has weathered into the crack. Where it crosses the felsitic beds it is not in any way incorporated with them, but for a short distance, a few inches, the felsitic bed has acquired a granitic structure which gradually dies away as the distance increases. The vein must be intrusive, and




from the closeness with which it fits to the adjacent rock can only have assumed its present position (not its crystalline texture) in a state of fusion. It might be supposed that the granitic character of the felsitic beds near it was due to fusion by the heat of the

intruded mass, but this cannot be, for in the first place the fusion would have produced an incorporation of the vein with the beds at the junction, which is not the case; and in the next place would have produced a uniform arrangement of the crystals in the beds, instead of which the beds have a different texture in the middle from that which they have at their upper and lower surfaces where they come in contact with the hornblende rock. The surfaces of the beds where they are in contact with the hornblende rock (which is quite unaffected but must have been subject to the same temperature) are not composed of the same material as the interior, but consist only of felspar and quartz, i. e. of felspar crystals with grains of quartz in them. The fact is that the vein, by affording easier communication both upwards and downwards, has contributed the conditions requisite for the crystallization of these beds, that is the freer permeation of air, water, steam and other substances, and concomitant variations of temperature.

In Alderney the lower strata are grey syenites closely resembling those of Guernsey, but overlaid by a stratified sandstone, consisting of felspar and quartz. It has probably been a loamy sand originally, and the particles of quartz in it look like original particles of sand, but the felspar is so highly crystalline that it must be called a metamorphic rock.

In Jersey the metamorphism of the rocks is quite as remarkable, but of a different kind. I have been unable to trace the connexion of the Jersey syenite, which is very different from that of the more northerly islands, with any stratified rocks. Ansted asserts that it may be done, but I had not time to explore the whole island. The Jersey syenites contain a large proportion of felspar, a variable proportion of quartz, and but little hornblende, and are generally coarsely crystalline, but in places pass into a very fine grained highly quartzose rock, or into a fine grained mixture of felspar and quartz, with larger crystals of felspar and lumps of quartz interspersed in a porphyritic manner, looking very much like a Cornish Elvan. This rock seems to form a sort of bed in the coarse syenite at St Brelade's, for it passes under the coarse syenite on the east side of the bay and above it on the west side. It is perhaps worthy of remark that in the processes of metamorphosis by which these and many other rocks have acquired either a porphyritic or syenitic structure the quartz does not assume the external form of crystals. It acts on polarized light as a crystal, but its external form is usually more or less rounded and granular, or looking as if it were pseudomorphic, as seen in section. This character is independent of the size of the crystals. In the coarsely crystalline parts of the syenite near St Brelade's, where the segregation of the felspar and quartz has been carried to the farthest extent, the quartz still appears in more or less rounded

lumps, while the felspar forms larger crystals. So in the porphyritic part, where the imbedded crystals of felspar are large, so are also the lumps of quartz. A good deal of Jersey is made up of shales and slates, but these are unconformable with the syenite. At Letacq, the northern extremity of St Owen's Bay, the syenite cuts through the shales right across the bedding. The shale at the junction is very hard and full of joints, and in some places has the stratification obliterated, and one might fancy that it had been altered by heat; but I found the shale in precisely the same condition elsewhere, where there was no syenite near to it; and moreover the shale is not altered all along the junction with the syenite, in places it is quite unaltered and adheres very closely to the syenite. The crack between the syenite and shale has been the condition determining metamorphosis so far as it has occurred. The shales vary in character in different parts of the island to a wonderful degree. At one place, La Crête point, a little north of Gorey, they pass into a sort of hornstone with a regular columnar jointing. Ansted calls attention to this spot. The stratification of the rock at this point is quite distinctly seen as a striping of the compact mass, and sometimes as regular layers of minute bubbles. The rock looks to me as if it had really been almost fused, and had acquired its columnar jointing, not a crystalline texture, in cooling.

I was unable to explore the whole island. Ansted remarks upon some conglomerates which I had no opportunity of examining; but I came across one which I think he does not mention. It forms part of the projecting headland at Plemont on the north-west of the island. It is stratified, and consists of rounded pebbles of all sizes from an inch to a yard in diameter. Most of these pebbles are of the syenite of the neighbourhood, but there are besides a good many pebbles of other rocks, many of a micaceous rock which I have not seen elsewhere in the island. The curious point about this conglomerate is that the matrix is granitic. It is in a somewhat rotten condition as if weathered, but still hard enough to stand much pounding by the sea, and under the hammer both matrix and pebbles break through together. The weathering of this matrix is quite of a peculiar character, the ordinary syenite separated by joints merely weathers away at the angles, but this granite weathers in channels and pits, taking fantastic forms like weathered limestone. It must be a re-made granite, but though the pebbles are arranged in strata it is plain that the materials of the matrix have not been assorted by water, for they are mixed quite as in ordinary granite. I believe the deposit to be an old sea beach which has been raised and then the pebbles have crumbled under the action of the weather, the crumbled parts have filled up the interstices and afterwards the whole has been

consolidated. That the pebbles may have crumbled down in this way is proved by the fact that the ordinary syenite whenever it comes to the surface is covered with a considerable depth of a rotten material of a similar character, and exposed lumps of syenite weather down to a rounded form resembling rolled pebbles.

The general conclusions to which my observations of these rocks point are, that the granitic structure is a metamorphic character which may be imparted either to stratified or to igneous rocks; but is not due to igneous fusion, or to any action which can occur only at a great depth in the earth. The names "plutonic" and "hypogene" applied to granitic rocks may be excellent on the lucus a non lucendo principle, but such rocks ought to be classed as metamorphic, and their igneous or aqueous origin determined by considerations entirely different from their crystalline characters.

A communication was made to the Society by

(2) Mr Bonney, *On the Rocks of the Lizard District* (*Cornwall*).

The author brought forward evidence to prove that the serpentine of this district was clearly intrusive among the hornblende schists. He then described the microscopic structure of the serpentine, and showed by comparison with the olivine of lherzolite, and of certain gabbros, that it was an altered olivine rock. The gradual conversion of olivine to serpentine, as exhibited in these instances and in slides selected from the Lizard rocks, was described. Enstatite and augite were frequent in the latter, with iron peroxides, as well as (occasionally) picotite. Hence the Lizard serpentine may be regarded as an altered lherzolite. The gabbros, which are intrusive in the serpentine, were then described. These are of two ages; the older, probably limited to Coverack Cove, has olivine partially converted into serpentine, but is otherwise not greatly changed. The newer (for that of Crousa Down is probably of the same age as the veins on the east coast) often has its pyroxenic constituent, and apparently the olivine also, to a large extent converted into minute hornblende, and its felspar into a hydrous mineral, which may be called saussurite. A brief description was given of the granite veins of the west coast, and of some metamorphic rocks on the east coast resembling granite. On the latter coast are found dykes of dark trap, the newest rocks of all; some are augitic, some hornblendic; but the writer thought that the latter mineral was probably an alteration product.

*November* 5, 1877.

PROF. CAYLEY, VICE-PRESIDENT, IN THE CHAIR.

The following communication was made to the Society by

Prof. CLERK MAXWELL, *On the unpublished electrical papers of the* Hon. HENRY CAVENDISH.

Cavendish published only two papers relating to electricity, "An attempt to explain some of the principal Phænomena of Electricity by Means of an Elastic Fluid" [*Phil. Trans.* 1771, pp. 584—677], and "An account of some Attempts to imitate the Effects of the Torpedo by Electricity" [*Phil. Trans.* for 1776, pp. 196—225]. He left behind him, however, some twenty packets of manuscript on mathematical and experimental electricity. These were placed by the then Earl of Burlington, now Duke of Devonshire, in the hands of the late Sir William Snow Harris, who appears to have made "an abstract of them with a commentary of great value on their contents." This was sent to Dr George Wilson when he was preparing his Life of Cavendish (*Works of the Cavendish Society*, Vol. I. London, 1851). It was afterwards returned to Sir W. S. Harris, but I have not been able to learn whether it is still in existence. The Cavendish manuscripts, however, were placed in my hands by the Duke of Devonshire in 1874, and they are now almost ready for publication.

They may be divided into three classes:

(A) Mathematical propositions, intended to follow those in the paper of 1771, and numbered accordingly. Some of these are important as showing the clear ideas of Cavendish with respect to what we now call charge, potential, and the capacity of a conductor; but the great improvements in the mathematical treatment of electricity since the time of Cavendish have rendered others superfluous.

We come next to an account of the experiments on which the mathematical theory was founded. This is a manuscript fully prepared for the press, and since it refers to the second part of the published paper of 1771 as "the second part of this Work," it must have been intended to be published as a book, along with a reprint of that paper. It contains no dates, but as it refers to experiments which we know were made in 1773, it must have been written after that time, but I do not think later than 1775.

It forms a scientifically arranged treatise on electricity. A

manuscript entitled "Thoughts concerning electricity" seems to form a kind of introduction to this treatise, for it contains several important definitions and hypotheses which are not afterwards repeated.

Next comes the fundamental experiment, in which it is proved that a conducting sphere insulated within a hollow conducting sphere does not become charged when the hollow sphere is charged and the inner sphere is made to communicate with it.

Cavendish proves that if this is the case, the law of force must be that of the inverse square, and also that if the index instead of being 2 had been $2 \pm \frac{1}{50}$, his method would have detected the charge on the inner sphere.

The experiment has been repeated this summer by Mr Mac Alister of St John's College with a delicate quadrant electrometer capable of detecting a charge many thousand times smaller than Cavendish could detect by his straw electrometer, so that we may now assert that the index cannot exceed or fall short of 2 by the millionth of a unit.

The second experiment is a repetition of this, using one parallelepiped within another instead of the two spheres.

He then describes his apparatus for comparing the charges of different bodies, or, as we should say, their capacities.

He first shows (Exp. 3) that the charge, communicated to a body connected to another body at a great distance by a fine wire, does not depend on the form of the wire, or on the point where it touches the body.

Exp. 4 is on the capacities of bodies of the same shape and size but of different substances.

Exp. 5 compares the capacity of two circles with that of another of twice the diameter.

Exp. 6 compares the capacity of two short wires with that of a long one.

Exp. 7 compares the capacities of bodies of different forms, the most important of which are a disk and a sphere.

Exp. 8 compares the charge of the middle of three parallel plates with that of the outer plates.

In the next part of his researches he investigates the capacities of condensers formed of plates of different kinds of glass, rosin, wax, shellac, &c. coated with disks of tinfoil, and also of plates of air between two flat conductors. He finds that the electricity spreads on the surface of the plate beyond the tinfoil coatings, and he investigates most carefully the extent of this spreading, and how it depends on the strength of the electrification.

After correcting for the spreading, he finds that for coated plates of the same substance the observed capacity is proportional to the computed capacity, but it is always several times greater

than the computed capacity, except in the case of plates of air, Cavendish thus anticipated Faraday in the discovery of the specific inductive capacity of dielectrics, and in the measurement of this quantity for different substances.

For these experiments Cavendish constructed a large number of coated plates with capacities so arranged that by combining them he could measure the capacity of any conductor from a sphere 12·1 inches diameter to his large battery of 49 Leyden jars. He expressed the capacity of any conductor in what he calls "inches of electricity," that is to say the *diameter* of a sphere of equal capacity expressed in inches.

The details and dates of the experiments referred to in this work are contained in three volumes of experiments in the years 1771, 1772 and 1773, in a separate collection of "*Measurements*" and in a paper entitled "*Results*," in which the experiments of different days are compared together. Besides these there are experiments of other kinds which are not described in the treatise.

The most important of these experiments are those on the electric resistance of different substances, which were continued to the year 1781.

He compares the resistance of solutions of sea salt of various strengths from saturation to 1 in 20000, and measures the diminution of resistance as the temperature rises. He also compares the resistance of solutions of sea salt with that of solutions containing chemical equivalents of other salts in the same quantity of water. He finds the resistance of distilled water to be very great, and much greater for fresh distilled water than for distilled water kept for some time in a glass bottle.

I have compared Cavendish's results with those recently obtained by Kohlrausch, and find them all within 10 per cent. and many much nearer.

Cavendish also investigates the relation between the resistance and the velocity of the current, and finds the power of the velocity to be by different experiments 1·08, 1·03, 0·976 and 1, and he finally concludes that the resistance is as the first power of the velocity, thus anticipating Ohm's Law.

The general accuracy of these results is the more remarkable when we consider the method by which they were obtained, forty years before the invention of the galvanometer.

Every comparison of two resistances was made by Cavendish by connecting one end of each resistance-tube with the external coatings of a set of equally charged Leyden jars and touching the jars in succession with a piece of metal held in one hand, while with a piece of metal in the other hand he touched alternately the ends of the two resistances. He thus compared the sensation of

the shock felt when the one or the other resistance in addition to the resistance of his body was placed in the path of the discharge. His results therefore are derived from the comparison of the sensations produced by an enormous number of shocks passed through his own body.

The skill which he thus acquired in the discrimination of shocks was so great that he is probably accurate even when he tells us that the shock when taken through a long thin copper wire wound on a large reel was sensibly greater than when taken direct. The experiment is certainly worth repeating, to determine whether the intensification of the physiological effect on account of the oscillatory character of the discharge through a coil would in any case compensate for the weakening effect of the resistance of the coil. But I have not hitherto succeeded in obtaining this result. Indeed on comparing the shock through two coils of equal resistance, one of which had far more self-induction than the other, I found the shock sensibly feebler through the coil of large self-induction.

*November* 19, 1877.

PROF. LIVEING, PRESIDENT, IN THE CHAIR.

A communication was made to the Society by

Prof. HUGHES, *On the base of the Cambrian Rocks in North Wales.*

Prof. Hughes read a paper in which he described the lower beds of the Cambrian Rocks near Bangor and Carnarvon. Among these he recognised the Arenig Beds, Lingula Flags, and Harlech Grits, in the lowest part of which were purple and green slates and, at the base, invariably a coarse grit and conglomerate.

Below the recognised Cambrian Rocks came a series of slates, breccias and porphyries, and these, where they could be followed, were found to rest upon quartz felsites which seemed to pass down into a coarse crystalline quartz-felspar rock which had variously been called Syenite, Syenitic porphyry, or felspar porphyry; a formation which as it stands looks like rocks usually known as igneous but which he thought might well be of metamorphic origin.

He had not found evidence of any discordancy betwen the older granitoid rocks of Carnarvon and the overlying quartz felsites, or between them and the green slates, hornstones, breccias, and porphyries of Bangor.

In the Bangor and Carnarvon district there was, he thought, much reason for suspecting that the conglomerate seen near and pierced by the east shaft to the railway tunnel was transgressive across the outcrops of the underlying rocks. Moreover it contained fragments of all the underlying rocks and of others which had not yet been identified.

He considered therefore that the conglomerate should be taken as the base of the Cambrian Rocks—that the Lingula Flags and Harlech Grits were thinning out to the North but would still be recognised, and that the Carnarvon and Bangor rocks below the conglomerate were a volcanic series.

## *December* 3, 1877.

PROF. LIVEING, PRESIDENT, IN THE CHAIR.

(1) The following communication was made to the Society by

Lieut. G. S. CLARKE, R.E., *On an optical method for investigating Rotary Motion.*

If a number of dots at equal intervals are viewed in a mirror, or through a lens attached to a tuning-fork, then, in virtue of the retention of their images on the retina, the dots will appear as straight lines when the fork is set in vibration. Thus the images of the dots shewn in Fig. 2 will appear drawn out into the lines shewn in Fig. 3 if the fork is so placed that the motion of the images is at right angles to a line passing through the dots.

If now a motion is given to the dots at right angles to the direction of their images, the two combined rectilinear motions produce the appearance of a sinuous line, or wave form.

The height of this wave will depend on the amplitude of vibration of the fork, while the wave length will depend on the relation of the speed of the dots to the period of the fork.

If certain exact ratios obtain between the velocity of the dots and the period of the fork, the waves formed will be absolutely stationary. If the velocity of the dots is slightly greater, or less than that required for the fulfilment of these ratios, the wave will be the same in form as that which the exact ratio would give, but it will have a slow progressive motion. This progressive motion will be in the same direction as that in which the dots move if the velocity of the latter is too great, and in the reverse direction if it is too small for the exact ratios.

The laws governing the ratios on which the formation of stationary waves depends may be stated as follows:—

If the velocity of the dots is such that the time occupied by each dot in passing over the interval between two adjacent dots is *exactly* equal to the period of one complete vibration, a single stationary wave (Fig. 4*a*) is produced.

If two intervals are traversed in the same time a compound double wave (Fig. 5*a*) will be obtained, and so on for compound triple and quadruple waves (Figs. 6*a* and 7*a*).

Similarly more complex forms may be produced. The eighth compound wave has been actually observed.

By varying the velocity of the dots, other waves can be obtained of the same *form* but of different wave lengths. Thus if *two* complete vibrations take place in the time of passing of a dot over *one* interval, a new single wave (Fig. 4*b*) is produced. Again, if *three* vibrations occur in the time of passing of a dot over *two* intervals, a second compound double wave (Fig. 5*b*) is produced, and so on. There is thus an infinity of series of compound waves and an infinity of waves in each series. The first or primary wave in each series requires the greatest velocity for its production and is the most brilliant.

The laws of formation of different waves may be shortly stated as follows:

(1) *Single wave,* any whole number of complete vibrations in the time of passing of a dot over *one* interval.

(2) *Double wave,* 1, 3, 5, 7, etc. complete vibrations in the time of passing of a dot over *two* intervals. Any whole number of vibrations not divisible by 2.

(3) *Triple wave,* 1, 2, 4, 5, 7, etc. complete vibrations in the time of passing of a dot over *three* intervals. Any whole number of vibrations not divisible by 3.

(4) *Quadruple wave,* 1, 3, 5, 7, 9, etc. complete vibrations in the time of passing of a dot over *four* intervals. Any whole number of vibrations not divisible by 2 or 4.

And so on for more complex forms.

The above laws may also be expressed as follows:—Denoting the velocity of the dots required for the formation of the primary wave of the first series by unity; the velocities required to produce other waves will be

| | |
|---|---|
| 1st Series | $1, \frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}$ . . . . . . |
| 2nd " | $2, \frac{2}{3}, \frac{2}{5}, \frac{2}{7}, \frac{2}{9}$ . . . . . . |
| 3rd " | $3, \frac{3}{2}, \frac{3}{4}, \frac{3}{5}, \frac{3}{9}$ . . . . . . |
| 4th " | $4, \frac{4}{3}, \frac{4}{5}, \frac{4}{7}, \frac{4}{9}$ . . . . . . |
| 5th " | $5, \frac{5}{2}, \frac{5}{3}, \frac{5}{4}, \frac{5}{6}$ . . . . . . |

It remains to show how the above principles can be utilized in a practical form. Selecting the primary double wave (Fig. 5*a*) as the one best adapted for measuring velocities, it will be evident that if

$$v = \frac{2t \times 60}{n};$$

where $n$ is the number of dots equally spaced round the perimeter of a drum, $v$ the number of rotations of the drum per minute, $t$ the number of complete vibrations of the fork, then a stationary primary double wave will be produced. From this equation $v$, $n$, or $t$ can be found for any given or assumed value of the other two. If, therefore, it were required to read velocities of from 20 to 40 revolutions per minute, it would be necessary to place round the drum a series of rings of dots containing

$$\frac{2t \times 60}{20}, \quad \frac{2t \times 60}{21}, \quad \frac{2t \times 60}{22}, \ldots\ldots\ldots$$

etc. dots each. If then any ring, say the 5th from the ring corresponding to a velocity of 20, gave the stationary double wave, it would be certain that the speed of the drum was 25.

A practical difficulty arises, however, in placing such a series of dots round a drum, for instance, with a fork vibrating 60 times a second, or reed, the perimeter of the drum would have to be divided into 342·8571 intervals, in order to obtain a stationary primary double wave with a velocity of 21 per minute, and moreover, even if such a division were practicable, it would not be possible to read off speeds intermediate between whole numbers of rotations per minute, *e.g.* $21\frac{1}{2}$. It becomes necessary, therefore, to adopt some other method.

If, instead of dots, equidistant lines are placed round a drum, parallel to its axis, and are observed through a slit attached to a fork, or reed, precisely similar waves to those above described are formed. A piece of paper is prepared by ruling lines as in Fig. 8, Plate II, which all converge in a point $o$, and pass through equidistant points on the line $ab$, and a rectangular portion $cdef$ is cut out, the size of the latter being just sufficient to wrap round the drum. These lines, when viewed through the slit, act as an infinite series of dots equidistant in each series, and exactly fulfil the requirements above indicated. Thus, if the convergent lines are so drawn that their intervals on $ce$ are such as to suit the maximum velocity $V$, and the intervals on $df$, the minimum velocity $v$ to be measured, then between $ce$ and $df$ there will be positions which will give stationary waves for every velocity between $V$ and $v$. Moreover, equal distances along the drum correspond to equal differences of velocities; thus, if $V$ is 60 and $v$ 20 rotations per minute,

the positions corresponding to velocities of 50, 40, and 30 will be found by simply dividing *cd* into four equal parts, and by further subdivision the positions corresponding to all velocities between $V$ and $v$ can be obtained.

In the Cycloscope, as at present constructed, a box *a* (Plate III.) containing a reed, to the tongue of which a piece of very thin zinc with a fine slit has been soldered, traverses on a slide in front of a drum carrying paper ruled in the way described.

The motion is given by a hand-wheel *b* to a pinion *c* on the same axis, the pinion gearing into a rack attached to the slide *d*. A pointer *p* attached to the reed box indicates on a scale *ss* the speed corresponding to any position of the reed box. A vernier might be employed in place of the pointer *p*, by which the primary divisions could be subdivided into 10 or 100 parts.

To make the waves more visible, two small lenses *e e* are employed, fixed in the front and back of the reed box. The lens on the back of the box throws an image of the lines on the slit; that in front magnifies this image, and thus parallax is avoided. If the instrument is to be used in a dark situation, a small lantern is placed on the bracket *t* attached to the side of the reed box, in order to illuminate the portion of the drum under observation.

In the early experiments it was found difficult to keep up a sufficient supply of air to the reed without a considerable pumping power and large conducting tubes. By the utilization of the principle of the injector, or jet pump, this difficulty has been entirely removed. The air is supplied through a small flexible indiarubber tube from a pair of foot-bellows. This tube terminates in a small glass tube drawn out to leave a narrow jet, $1\frac{1}{2}$ mm. in diameter. This fine jet is passed through a cork, fitted into a wide brass tube *k* (Plate III.) fixed into the lower part of the back of the reed box. The lower part of the brass tube is cut away to allow free access to the surrounding air. The arrangement is shown in enlarged section in Fig. 10, Plate II.

Air, of pressure about equal to a column of water 20 or 25 cm. in height, is forced through the jet, and the reed vibrates perfectly, the mean pressure of air in the reed box being only about equal to a column of water $1\frac{1}{2}$ mm. in height.

The graduation of the scale should be performed after the paper is mounted on the drum. The latter may be conveniently arranged so that the centre line *no* (Fig. 8) falls opposite the junction, which will then present the appearance shown in Fig. 9, but this is not essential. The lines must now be counted round at any two intersections and the period of the reed (60 complete vibrations per second) being known, the velocities which are required to produce stationary waves at these two positions can

be calculated by means of the equation given. Two divisions on the scale being thus obtained, the remaining divisions are found by subdivision.

Referring to Fig. 9 it will be evident that only at the intersections and at one intermediate point between each pair will circles traced round the drum be divided into parts, all of which are equal.

Circles traced at other positions will have one unequal division at the line of junction, which will cause a slight abrupt movement, or jump of the wave at each rotation of the drum. This jump however cannot be mistaken for the steady progressive motion which indicates that the position corresponding to the stationary wave has not been arrived at.

To make a reading it will be necessary to start the reed and to move the hand-wheel *b* until a stationary double wave is seen through the lenses *e e*, the pointer will then indicate the speed of the drum.

The reading can be made without taking the eye from the lens, if a scale is placed close in front of the drum at such a height that its graduations can be seen through the slit simultaneously with the waves. When such a scale is employed, it is not necessary to bring the reed or fork close to the drum. The prepared paper might in fact be wrapped round a distant shaft, and the reading can be made through a small telescope in the focus of the object-glass of which the slit is made to vibrate.

In order to set tuning forks in vibration the arrangement shown in Fig. 11 was devised, and has proved completely successful. A piece of soft iron turned up at the ends is carried on an axis between the prongs of the fork, the axis being turned up to form a handle. By turning this handle till the soft iron bar is in the position shown, the prongs are forced apart. If then the handle is again sharply turned in the opposite direction the fork is set in vibration.

It has so far been assumed that the period of a fork or reed is absolutely constant. This is not the case, as the fork varies slightly with temperature, vibrating more slowly as the metal becomes warmer. In some experiments made with tuning forks a loss ·011 per cent. per 1° Centigrade was observed. This would be too small to affect the value of the instrument for practical purposes, while, if it were employed for delicate investigations, a correction could readily be applied. Temperature similarly affects reeds, their period is also lengthened by an increase of pressure of the air by which they are set in vibration. By the employment of the air injector described in a foregoing paragraph, any considerable variation in the pressure of the air supplied is prevented. The mean of 22 fairly concordant observations

gave ·010 per cent. for each degree Centigrade as the loss occasioned by rise of temperature. The determinations of the effect of temperature above alluded to were carried out by means of Lissajous' figures, and it may be mentioned that the latter can be produced by attaching pieces of paper with very fine slits to the forks in place of mirrors, as has been the usual practice. One fork is placed horizontally, the other vertically, a fixed lens being mounted between them so as to form an image of one slit on the other. With large forks the effect of the additional weight of the paper and gum is inappreciable.

There are several methods of determining the absolute period of a reed or fork. These are described at some length in a paper read by Mr A. J. Ellis, F.R.S., before the Society of Arts in May, 1877.

The principle of the Cycloscope is now being employed in the construction of an apparatus for determining the absolute pitch of a fork or reed, and the experiments so far have given excellent results. If a steady rotary motion capable of perfect control is given to a drum carrying a ring of lines, and if the motion is so regulated that any recognized wave, given by a fork or reed of unknown period, is kept stationary during a measured interval of time, the exact number of rotations of the drum during this interval being accurately recorded, then evidently the period of the fork or reed can be obtained. It is not even necessary to attach a slit to the fork, as the edge of the latter is found to answer equally well.

Three determinations of the period of a 256 fork made by Prof. McLeod gave the numbers 256·287, 256·281 and 256·287 vibrations per second; a 320 fork gave 320·364; a 384 fork 384·456, and a 512 fork 512·549. These numbers must be regarded as merely preliminary, since known imperfections exist in the apparatus.

Some interesting experiments have been made with discs on the principle of the thaumatrope. If a disc provided with radial slits is driven at a constant speed by clockwork in front of another disc driven from any machine and provided with a ring of dots or symbols, and if $N$ and $n$ are the number of rotations of the clock disc and the other disc per minute respectively, $S$, the number of slits, $d$ the number of dots or symbols; then when $N . S = n . d$; $d$ dots will be visible and stationary. Thus $S$ and $N$ being given or assumed, $d$ can be obtained for any assigned value of $n$.

If the machine disc is running a little too fast for the above equation, the dots will appear to move slowly in the *same* direction as this disc; if too slow, they will move in the opposite direction.

If $N.S = 2d.n$; $2d$ dots will be visible and stationary.

If in place of dots symbols alternately alike are employed thus:—

/ \ / \ /

then, by reason of the superposition of adjacent symbols, when $SN = 2dn$, the appearance is $2d$ stationary,

X X X X X

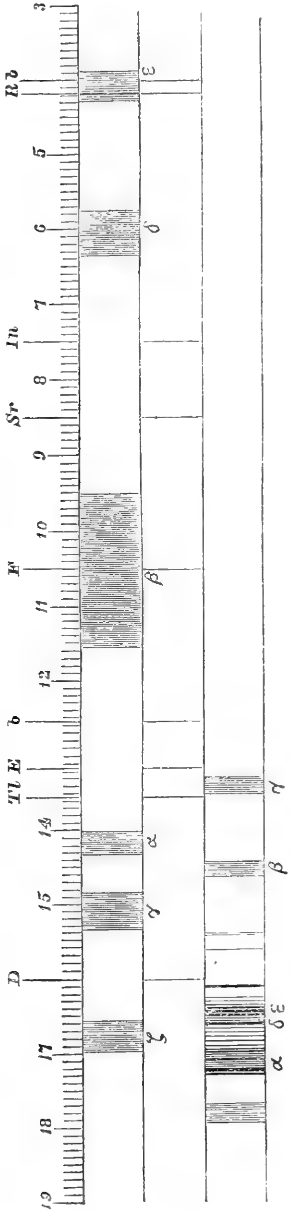
where $d$ is the *whole* number of symbols employed. Although the difficulty of obtaining a trustworthy standard rotary motion will probably prevent this method from being employed for the *absolute* measurement of velocities, there is no reason against its adoption for the investigation of the *relative* speeds of two machines.

To all cases where it is necessary to study carefully the working of a machine, the method of the Cycloscope can be applied with advantage, while its great elasticity permits it to be adapted to high or low speeds, long or short ranges of velocity, heavy engine machinery or light clockwork with equal facility.

(2) A communication was made to the Society by

Prof. Liveing, *Note on the spectra of Calcium Fluoride.*

No. 1 in the annexed woodcut is the diagram of the spectrum of fluor spar rendered phosphorescent by heat. I have examined several specimens, and all when they begin to be luminous shew a rather narrow band ($a$) in the green, and a broad band ($\beta$) extending into the green and blue about equal distances on either side of Fraunhofer's line F. Common nearly white or pale green fluor does not shew any more bands, but as the temperature rises these two bands widen and nearly meet, Derbyshire blue-john shews as the temperature rises a second green band ($\gamma$) on the yellow side of the first, and then another ($\zeta$) in the orange. The brighter-coloured green fluor, well known for its phosphorescence, shews also at the higher temperature two bands ($\delta$) and ($\epsilon$) in the purple and violet. In every case the bands ($a$) and ($\beta$) appear first and are the most persistent. The light is so faint that it is impossible to see any scale or micrometer distinctly at the same time as the bands, but I have taken the position of the bands of the bright green fluor approximately. The position of the first green band ($\alpha$) does not appear to be identical in all the specimens, but it is always in the green and on the yellow side of the thallium green line. I have not met with any specimens which shew the spectra described by Kindt



3
Rb
ε
5
6
δ
7
In
8
Sr
9
10
F
β
11
12
b
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γ
14
α
β
15
γ
D
ε
δ
ζ
17
α
18
19

in Poggendorf's *Annalen*, cxxxj. p. 160; but the spectra described by Becquerel as produced by fluor when exposed to the red rays of the solar spectrum, and attributed by him to the action of heat, agree with the bands I have marked ($\alpha$) and ($\beta$).

No. 2 is the diagram of the spectrum produced by fluor moistened with hydrochloric acid in the flame of a Bunsen's burner: when a solution of fluor in nitric acid is used only the bands marked ($\alpha$), ($\beta$), ($\gamma$) and that from ($\delta$) to ($\epsilon$) are seen, the other lines being probably due to the chloride. I have thought it worth while to call attention to this spectrum, because in analysing silicates with the spectroscope it is frequently necessary to decompose them with hydrofluoric acid or alkaline fluorides, and the ordinary text-books do not indicate the existence of the bright green band ($\gamma$) which is very characteristic. Mr Schuster has reminded me that A. Mitscherlich has described the spectrum of calcium fluoride (Pogg. cxxj.); but his diagram and description do not give an adequate idea of the spectrum. He makes the band ($\gamma$) consist of two about equal and quite distinct parts. It is true that it is divided in two by a fine dark line at about one-third of the breadth of the band from the violet side, but the division is only just visible with such spectroscopes as are ordinarily used for chemical analysis: and it is to be noted that the other green band ($\beta$) is divided nearly in the middle by a dark line more easily visible. These dark lines are, however, both too fine to be represented in the woodcut.

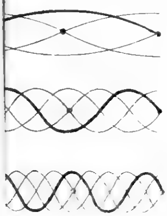
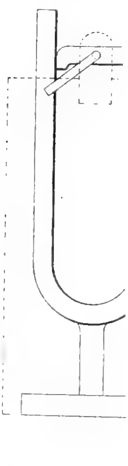
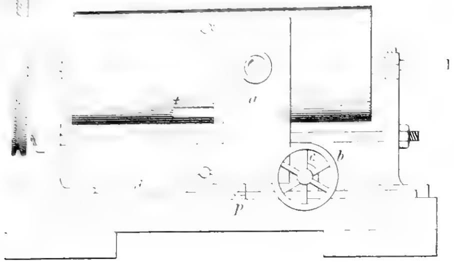


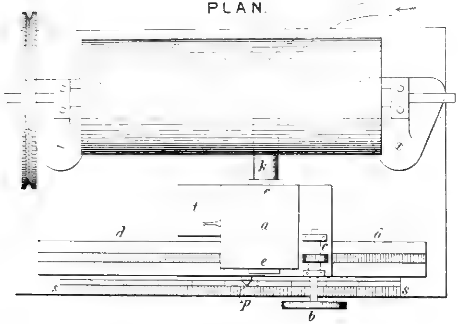

Fig. 1



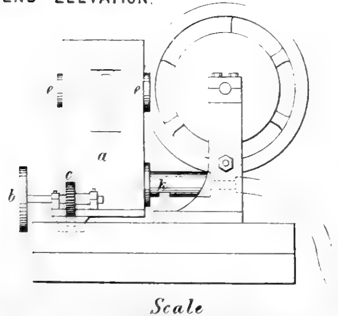
FRONT ELEVATION



PLAN.



END ELEVATION.



Scale
2" to 1'·0"




Fig. 7.

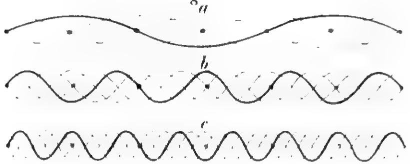


Fig. 8

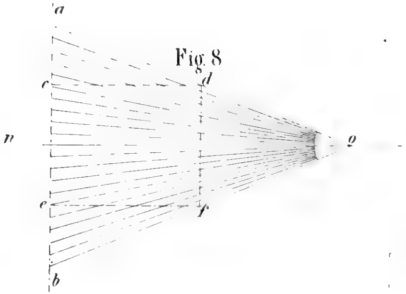


Fig. 9.

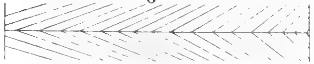


Fig. 10.

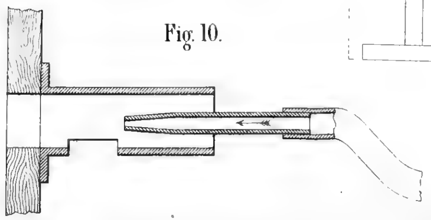


Fig. 11

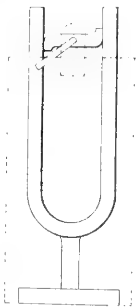




Fig. 1.

Fig. 2.



Fig. 3.

Fig. 4.

Fig. 5.

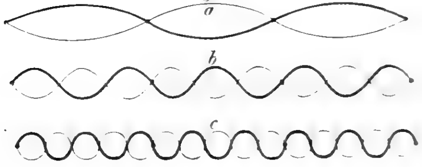
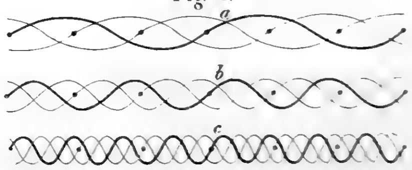


Fig. 6.



# PROCEEDINGS

OF THE

# Cambridge Philosophical Society.

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*February* 11, 1878.

PROFESSOR LIVEING, PRESIDENT, IN THE CHAIR.

The following communication was made to the Society:—

Mr J. W. L. GLAISHER, M.A., F.R.S., *On factor tables, with an account of the mode of formation of the factor table for the fourth million*[1].

§ 1. If it be required to determine whether a given number is prime or not, the only available method (in the absence of tables) is to divide it by every prime less than its square root, or until one is found that divides it without remainder[2]. If, on the other hand, the object is to form a table of prime numbers, the process is theoretically very simple and rapid. For, suppose all the natural numbers ranged in order in one straight line at equal distances, thus:

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 ...

[1] This paper has been considerably amplified since it was read to the Society. J. W. L. G. (June, 1878). A table of contents is given at the end of the paper (p. 137).

[2] Euler commences his memoir "Quomodo numeri præmagni sint explorandi, utrum sint primi, nec ne" (*Opera minora collecta*, St Petersburg, 1849, t. I. pp. 379—390) as follows: "Ante omnia monendum est, me hic non ejusmodi methodum polliceri, cujus ope omnes omnino numeri, cujuscunque sint generis, examinari queant, utrum sint primi, nec ne? Hujusmodi enim methodum vix aliam dari posse existimo, nisi quae ad regulam redeat vulgarem, qua divisio per omnes numeros primos, radice quadrata numeri propositi minores, est tentanda, quæ operatio sane, si numeri saltem mediocriter magni proponantur, nimis est molesta, quam ut suscipi queat." Euler's method relates only to primes of the form $4n+1$ which as is well known are expressible as the sum of two squares in one, and only one, way.

Start from 2, and strike out every second number; we thus reject all the multiples of 2, except 2 itself; start from 3, and strike out every third figure, and we thus reject multiples of 3, and similarly for 5, 7, 11, &c. In this way, all the numbers not divisible by a factor less than any given prime $p$ can be found; or in other words, we thus form a list of primes up to $p^2$. The same process, which is quite mechanical, also gives all the prime factors of each composite number. For, take a pair of compasses, and open them to such an extent that the points are separated by a distance equal to twice the distance between two consecutive numbers; if, starting with the number 2, the compasses be 'stepped' along the line of numbers, we have all the multiples of 2 marked, and have only to write down 2 opposite to each number; similarly, if the points of the compasses be separated by a distance equal to 3, we have all the multiples of 3 marked, and so on. In this way we obtain a factor table as shown below, the primes being distinguished by their having blanks opposite to them[1]:

| | | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | 3 | | | | | | 3 | | 7 | 5 | | | 3 | | 5 | 7 | ... |
| | | | 2 | 5 | 2 | | 2 | 3 | 2 | | 2 | | 2 | 3 | 2 | | 2 | | 2 | 3 | |
| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 | ... |

This is the principle of the mode of construction of all factor tables and lists of primes; but the process is very greatly modified in the actual methods that have been used. In practice the compasses could not be employed, as the natural numbers must be arranged in columns, instead of being written in one straight line, but methods have been devised in which their use is replaced partly by measurements and partly by calculations.

In the case of large factor tables, numbers divisible by 2, 3, or 5 are omitted, so that every $p$th number of those in the table is not divisible by $p$. The law regulating the occurrence of the multiples of $p$ is still periodic, but is more complicated, and it is convenient to use, for each prime $p$, a screen or sieve, *i.e.* a piece of paper or card, of the proper size, from which certain squares have been cut out according to such a law, that when suitably laid upon the columns of squares representing the arguments of the table, those squares which correspond to multiples of the number $p$ shall appear through the holes in the sieve: and the factor $p$ is accordingly to be entered in each square, or in each empty square, that appears through the sieve, according as

[1] Considering only the theory of the table, it would be better, in using the compasses, to always start from unity, so that a prime would be recognised by its having only one factor opposite to it, and that factor equal to the number: but, practically, the insertion of these factors in the table would be useless, and very inconvenient.

all the prime factors, or only the least factors of the numbers, are to be recorded in the table.

Assuming the necessity for occasionally requiring the factors of a large number, it will be seen that the existence of a factor table of considerable extent is a very important matter; for the process of determining, without a table, the factors of a number, unless one of them happens to be small, or of proving a number to be prime, is excessively laborious; the process is also an unsatisfactory one, as in the event of no factor being found that divides the number without remainder, a duplicate calculation will be required in order to render it certain that no error has been made in the divisions. Thus to determine, for example, whether the number 8,559,091 is or is not a prime would require a long day's work, as the number is a prime; and even then the determination would be less satisfactory than if the result were obtained directly from a table.

The intrinsic value of a table, quà table, may be regarded as proportional to the actual amount of time saved by the table, whenever there is necessity for consulting it: thus, *ex. gr.* a table of square roots to 10 decimals is intrinsically more valuable than a table of squares, as the extraction of the root would occupy more time than the multiplication of the number by itself[1]. There are not many tables that exceed a factor table in intrinsic value. It should be remarked also that the intrinsic value of a factor table is not appreciably diminished if only the least factor be given, instead of all the factors, for we have only to divide the number by the least factor, and enter the table with the quotient as argument, and so on. It is more convenient in some cases to have all the factors given, but the purpose for which the table is constructed is equally fulfilled by giving the least factor as by giving all, and the saving of space, when room has to be found for one factor only, is very great.

[1] The fact of whether a table is easy or difficult to construct does not affect its intrinsic value when completed. When a table is made, it is made for ever, and as far as the user is concerned it is indifferent whether its construction occupied the original calculator for one year or ten years. The value of a table of logarithms, for any given person, consists in the amount of time he saves by its use; and the work originally devoted to the calculation of logarithms two centuries and a half ago in no way concerns him.

Referring to factor tables, Lambert wrote: "Universalis finis talium tabularum est: *ut semel pro semper computetur, quod sæpius de novo computandum foret; et ut pro omni casu computetur, quod in futurum pro quovis casu computatum desiderabitur.*" (*Supplementa Tabularum*, 1798, p. xv.)

The theory of the construction of a factor table is simple, but in practice the table is not an easy one to form, the chief difficulty consisting in the *incessant* care required to avoid making errors, (as an error made, cannot be readily discovered and corrected), and the absence of any complete method of verification, such, for example, as is supplied by differences in the case of a table containing the values of a continuous quantity.

It follows from the description given above that any method of constructing a factor table affords all the prime factors, so that as far as the actual calculation of the table is concerned, it is as easy to give all the prime factors as to give the least. A little time, in actual writing, may be saved by entering only the least factor, but this is counterbalanced by the much greater ease with which a table containing all the prime factors can be verified while in course of construction.

§ 2. There are several cases in which the factors of a number may be of use in ordinary mathematical processes; as for example, in the calculation of logarithms: but the real importance of an extended factor table lies in the fact that such a table is a fundamental one in the Theory of Numbers. The number of factors of a number, their sum, &c., are elements which enter into many results in the Theory of Numbers, and it is clear that, even on this account alone, it is desirable to have a table by means of which the resolution of high numbers into their factors is rendered practicable, so that suitable examples, verifications, &c., of such theorems may be obtainable. Also, conjectural theorems of this nature could not readily be tested without such tables. Again, the law of frequency of prime numbers has been the subject of analytical investigation, and theoretical formulæ giving the average frequency of primes, and the approximate number of primes between any given limits, have been obtained by Legendre, Tchébychef, Hargreave, Riemann and others, and it is essential to have the means of comparing the numbers given by these formulæ with those found by actually counting the primes[1]. Such asymptotic formulæ cannot be satisfactorily tested by means of enumerations made near the beginning of the series of numerals, for the slightest consideration shows how anomalous is the distribution of primes at first: in fact, if we possessed a complete factor table from 1 to 10,000,000, this would be found to be only barely sufficient to afford comparisons of real value.

The practical use of a factor table in obtaining logarithms has been alluded to at the beginning of this section. This, although not the *raison d'être* of such a table, is so important an application as to deserve special notice, for by its means the number of numbers whose logarithms are known is greatly extended. For example, Abraham Sharp's table contains 61-decimal Briggian logarithms of primes to 1,100, so that the logarithms of all numbers whose greatest prime factor does not exceed this number,

[1] References to these investigations are given by Professor H. J. S. Smith in his address, "On the present state and prospects of some branches of pure mathematics." *Proceedings of the London Mathematical Society*, Vol. VIII. (no. 104) pp. 16—19, (1876—1877).

may be obtained by simple addition. Similarly, Wolfram's table gives 48-decimal hyperbolic logarithms of primes up to 10,009. Thus a factor table forms a very valuable complement to many-place logarithmic tables, the range of which must necessarily be comparatively limited; and even in the case of large prime numbers, or numbers exceeding the limits of the factor table, it affords a very convenient method of calculating logarithms. An example to illustrate this application of the table is given in an appendix to this paper (§ 21).

But, independently of these applications of the table, the resolution of a number into its factors is so elementary and fundamental an operation, but withal so difficult in individual cases, that a factor table may well be regarded as both a collection of valuable facts, and as a table interesting in itself. It is not surprising, therefore, that the formation and extension of such tables have long engaged the attention of mathematicians, and that a great amount of labour has been devoted to this object.

§ 3. The following works, &c., are described by Chernac in Part III. of the Introduction to his *Cribrum Arithmeticum* (1811).

1657. Francis Schooten. List of primes to 10,000.

1668. Pell (in Brancker's translation of Rhonius's Algebra, published at London). Least divisors of numbers not divisible by 2 or 5, to 100,000.

1717. De Traytorens. Exhibits to the French Academy a method of making factor tables.

1728. Poetius (in his Arithmetic, Leipzig). An 'anatome' of numbers to 10,000. Reprinted by Richter in t. II. of his mathematical Lexicon.

1746. Krüger. Table of primes to 101,000[1]. Calculated by Peter Jäger.

1767. Anjema. Factor table to 10,000. All divisors (prime and composite) given. Table occupies 302 quarto pages. This was a posthumous work. Anjema had intended the table to extend to 100,000.

1768. Rallier Des Ourmes. Communicates to the French Academy a method of finding factors of numbers.

1770. Lambert (in his *Zusätze zu den...Tabellen*, Berlin). Least factors of numbers not divisible by 2, 3, or 5 to 102,000; and list of primes to 102,000.

1772. Marci. List of primes to 400,000.

1774. Euler. Memoir on the construction of a factor table to extend to a million.

1778. Bertrand. Remarks on the formation of factor tables.

[1] The title-page has "Primzahlen von 1 bis 1000000" but the list ends with 100,999. (Brit. Assoc. Report, p. 35.)

1785. Neumann. All prime factors of numbers, not divisible by 2, 3, or 5, to 100,100. Table occupies 200 quarto pages.

1797. Vega. All prime factors of numbers, not divisible by 2, 3, or 5, to 102,000; and a list of primes from 102,000 to 400,000.

1804. Krause. Factor table to 100,000. Table occupies 28 folio pages.

In preparing the British Association Report on Tables[1], I met with the following tables published previous to 1811, which are not included in the above list.

1659. Rahn. Least factors of numbers, not divisible by 2 or 5, to 24,000. Rahn is the same as Rhonius referred to under Pell, 1668.

1745. Dodson. Least factors of numbers, not divisible by 2 or 5, to 10,000.

1758. Pigri. All prime factors of numbers to 10,000.

1795. Maseres. Least factors of all numbers, not divisible by 2 or 5, to 100,000. Reprint of Brancker's table. See Pell, 1668.

1798. Gruson. Prime factors of numbers, not divisible by 2, 3, or 5, to 10,500. And I should also mention

1800. Snell. Factor table to 30,000. This work I have not seen, but only the title "Snell (F. W. D.), Ueber eine neue und bequeme Art, die Factorentafeln einzurichten, nebst einer Kupfertafel der einfachen Factoren von 1 bis 30000. 4to. Giessen und Darmstadt, 1800" (Brit. Assoc. Report, p. 35).

There are no doubt many other tables having as much claim to be included in the preceding list as some of those mentioned, but it is probable that the list contains all the more important factor tables and lists of primes published previous to 1811[2].

It may be remarked that a table showing all the prime factors of a number usually gives also their powers, *ex. gr.* opposite 4,932 we should find $2^2 . 3^2 . 137$; but in some tables only the prime factors, without the powers, are given. On account of the spaces, in which the factors of each number are contained, being necessarily of uniform size, it is not possible in many cases to give all the factors at length: thus to save room in Vega's table, $a, b, c, d$ are printed for 11, 13, 17, 19, and in Lambert's *Supplementa* (1798) $f, g, \ldots z, A, \ldots Q$ are printed for 11, 13,...89, 97...173. See § 11.

In a letter printed in his *Deutscher Gelehrter Briefwechsel*, t. v.

[1] *Report of the forty-third meeting of the British Association*, 1873, pp. 1—175. Art. 8. *Tables of Divisors (Factor Tables) and Tables of Primes*, pp. 34—39.

[2] It does not seem worth while to continue the list of the smaller tables beyond 1811: some are referred to in the British Association Report. Perhaps of the small tables, that contained in the original edition (1814) of Barlow's *Mathematical Tables* which gives prime factors and their powers for numbers to 10,000, is the most convenient and accessible. The column containing this table is omitted in the later (stereotype) editions.

p. 10, Lambert distinguishes between a factor and divisor as follows: "I understand by factors those primes, or their powers, which multiplied together give the number, *ex. gr.* $10{,}829 = 7^2 . 13 . 17$. These factors have the advantage that all the other divisors (Theiler) can be easily found, for $7 . 7 = 49$, $7 . 13 = 91$, $7 . 17 = 119$, $13 . 17 = 221$, $7 . 7 . 13 = 637$, $7 . 7 . 17 = 833$, $7 . 13 . 17 = 1{,}547$ are the other divisors." This definition does not appear to be very generally followed, and factor and divisor seem to be used merely to denote any number that divides the given number without remainder.

§ 4. Chernac at the end of his historical notice upon factor tables states that he has not been able to see Felkel's table, but that Krause states that letters of the alphabet are employed in it to denote the divisors. The table is so curious and rare, and, besides, the facts connected with its calculation and publication are so remarkable that I here give an account in some detail.

Of the work itself I have seen only two copies; one of which belongs to the Royal Society, and the other to the Graves Library, at University College, London. The former has a German title-page. "Tafel aller Einfachen Factoren der durch 2, 3, 5 nicht theilbaren Zahlen von 1 bis 10 000 000. Entworfen von Anton Felkel, Lehrer an der k. k. Normalschule. I. Theil. Enthaltend die Factoren von 1 bis 144000. Wien, mit von Ghelenschen Schriften gedruckt 1776;" the introduction is in German, and is dated "Wien den 30. September 1776." There are 26 large folio pages of tabular matter, giving all the prime factors of numbers up to 144,000. The Graves copy has the Latin title-page, "Tabula omnium factorum simplicium, numerorum per 2, 3, 5 non divisibilium ab 1 usque 10 000 000. Elaborata ab Antonio Felkel. Pars I. Exhibens factores ab 1 usque 144000. Vindobonæ, ex typographia a Gheleniana 1776." The introduction is also in Latin, and is dated "Viennæ, die 1 Aprilis, 1777." This is followed by 58 pages of tables, giving the least factors of numbers up to 336,000; and then occurs a fresh title-page, "Tabula factorum. Pars III. Exhibens factores numerorum ab 336001 usque 408000." [The figures '336' and '408' are evidently printed over the figures '144' and '336' which have been partially obliterated, and the first stroke of the III. in 'Pars III.' is a little out of its place, and seems to have been added subsequently.] After the title-page to part III. there are 12 pages of tables, giving the factors of the numbers from 336,000 to 408,000.

The contents of the German and Latin introductions are substantially the same, but the latter contains an errata-list which does not appear in the former.

I now proceed to describe the tables themselves. There are three tables: Table A (one page), Table B (one page), and the principal table extending to 144,000 or 408,000, and occupying 24 or 68 pages respectively in the two copies. Table A contains a list of primes up to 20,353; along the top line of the page run the Greek letters $\alpha, \beta, \ldots \chi$, and down the extreme left-hand column four alphabets in order, viz. small italic, small German, capital italic and capital German (there being 100 lines); any prime contained on this page is henceforth in the book denoted, as it were, by its coordinates; thus 4,721 is denoted by $\zeta\mathfrak{m}$, 17,191 by $\tau\mathfrak{F}$, &c. The first column has no Greek letter as a heading, so that the primes up to 523 are denoted each by a single letter.

Table B, and also each page of the principal table, contains 100 lines, the lines being numbered 1, 2, 3, ... 100 down a narrow column in the middle; the page is thus divided into two equal portions, each of which contains eight columns headed a, b, c, d, e, f, g, h. Table B (headed *Tafel der auf jeder Seite wiederkehrenden Endesziffern*) contains the numbers not divisible by 2, 3 or 5, up to 6,000, arranged in order under the eight columns of each half-page, viz. in the top line, left-hand side, 1 in column a, 7 in column b, 11 in column c, 13 in column d, and so on up to 5,999, which occupies the 100th line of the right-hand side in column h. This table may be said to contain the arguments for the principal table.

The principal table consists, as it were, only of tabular results, but each half-page may be divided into three blocks, each block being denoted by an argument-number (and also, for a reason that will appear further on, by a symbol involving a letter). These argument-numbers of the blocks run consecutively from 0 to 407, the block denoted, say, by 77 referring to the numbers between 77,000 and 78,000.

The mode of using the table is as follows: Suppose it required to find the factors of 138,593. Divide 138 by 6, the remainder is 0; enter table B at 0593, that is at 593; this number occurs in column g, line 20. Now turn to the principal table and enter it at block 138, column g, line 20; we find as tabular result $e$, $g$, $\beta\mathfrak{r}$, which interpreted by table A, gives 7, 13, 1,523 as the prime factors of 138,593. As a second example, suppose the factors of 141,793 required. Divide 141 by 6; the remainder is 3. Enter table B with 3,793; the result is column d, line 27. Enter the principal table with block 141, column d, line 27, and we find $793 - S$. The occurrence of the three last figures of the number followed by the hyphen indicates that the number 141,793 is prime, and as the block 141 is denoted by the symbol $\Lambda$), the prime 141,793, when it occurs as a tabular result in the table, will be denoted by $\Lambda$) $S$.

If, therefore, we had met with $\Lambda) S$ among the tabular results (as would first have been the case with the number 992,551 had the table extended so far), we should have looked for the block $\Lambda)$, which we should have found to be the block 141, and looking for $S$ in this block we should have found $793 - S$, showing that $\Lambda) S$ denoted the prime 141,793. Thus for primes above 20,353 the table has not only to show that the number is a prime, but also to define it by two letters, and this in such a way that if the letters be given, we can readily obtain from them the corresponding prime.

On the first page of the principal table (1 to 6,000) the last three figures of the argument are always given with the tabular result (the primes are distinguished by the three figures being followed by a hyphen, and also by their having only one letter given as tabular result), so that this first page might be used in place of table A; also the contents of table A are, in fact, included in the principal table if we choose to use it to find the primes represented by the pairs of letters up to 20,353 in the same way that it is used when the prime exceeds this number.

The method of arrangement is very ingenious, but it will be seen that the mode of entry is complicated, and that as the factors need interpreting afterwards there is great danger of error. There are four operations needed, (1) divide by 6, (2) enter table B, (3) enter the principal table, (4) interpret the factors. Also, as the table proceeds, the symbols for the primes become embarrassing, as when all the simple combinations are exhausted, brackets, &c. are used as in $(\zeta\mathfrak{H}$, $[\Gamma]\, a$, $(\xi)\, \mathfrak{B}$. The appearance of the pages of the table, containing figures and letters from so many alphabets in juxtaposition, is very remarkable. The arrangement in the eight columns a, b, ... g, h depends on the fact that all numbers not divisible by 2, 3 or 5 must be of the form $30q + r$, where $r = 1, 7, 11, 13, 17, 19, 23$ or $29$, the a column containing numbers of the form $30q + 1$, the b column numbers of the form $30q + 7$, &c.; and this explains the division by 6. A division by 3 would suffice, but the division by 6 shows in addition on which half of the page the result will be found in the principal table. The pages of the principal table are numbered from 1 to 24, corresponding to the numbers 1 to 144,000, and then the half-pages are numbered 1 to 88, corresponding to the numbers 144,000 to 408,000[1].

[1] The arrangement in eight columns headed 1, 7, 11, 13, 17, 19, 23, 29, so that the first column contains multiples of the form $30q + 1$, the second multiples of the form $30q + 7$ &c. is also that adopted by Euler in his memoir: "De tabula numerorum primorum, usque ad millionem et ultra continuanda; in qua simul omnium numerorum non primorum minimi divisores exprimantur." (*Opera minora collecta* vol. II. pp. 64—91.) The memoir originally appeared in vol. XIX. of the *Novi Commentarii Petropolitani* (1774), but I do not know whether the arrangement—a very natural one for a table from which multiples of 2, 3 and

The headings of tables A and B, and of the principal table, as far as 144,000, are in German in both copies; but the headings of the portion 144,000 to 408,000, in the Graves copy, are in Latin.

In the Graves copy table B follows table A and precedes the principal table, and in the other copy table B is placed at the end, after the principal table.

On the title-page of the work is a large engraving representing Felkel turning in contempt from a disordered cabinet of books of general literature[1], Basedow, Curtius, Gottsched, &c., to a neatly arranged cabinet of mathematical books, Euler, Kästner, Newton, Maclaurin, &c., the book which is open in his hand being Lambert. An apparatus, consisting of eight rods in a frame, is resting against the table; this is no doubt Felkel's machine for forming a factor table, referred to hereafter.

On a scroll over the top is written "(Gramma)tici certant, geometræ vera seqūntur. Bella odi, pacem diligo, vera sequor." The beginning of the first word is hidden by a fold of the scroll, 'tici' alone being visible, but the word is probably *Grammatici*, in allusion to the line, 'Grammatici certant et adhuc sub judice lis est' (Horace, *Ars Poetica*, 78).

§ 5. At the time of writing the British Association Report I knew nothing of Felkel except what he states himself in the Latin edition of Lambert's *Zusätze zu den Logarithmischen und Trigonometrischen Tabellen* (Berlin, 1770), which was translated into Latin by Felkel, after Lambert's death, and published at Lisbon in 1798, under the title, *J. H. Lambert supplementa tabularum logarithmicarum et trigonometricarum auspiciis almæ academiæ regiæ scientiarum Olisiponensis cum versione introdutionis* (sic) *Germanicæ in Latinum sermonem, secundum ultima auctoris consilia amplificata. Curante Antonio Felkel. Olisipone...* MDCCXCVIII. He there states that he had continued his table to 5,000,000 (this statement will be referred to in § 11). I also quoted Gauss's remark which occurs in his letter prefixed to Dase's *Seventh Million* (see § 17), viz. "Felkel hatte die Tafel in Manuscripte bis 2 Millionen fertig und der Druck war bis 408000 fortgeschritten, dann aber sistirt, und die ganze Auflage wurde vernichtet bis auf wenige Exemplare des bis 336000 gehenden Theils, wovon die hiesige Bibliothek eines besitzt.'

Recently, however, I found that a great deal of light was

5 are excluded—occurred to Felkel independently, or was suggested to him by Euler's writings. Hindenburg in a letter to Lambert of December 22, 1776, draws his attention to Euler's arrangement and compares it with Felkel's (*Briefwechsel*, v. pp. 200, 201).

[1] In the *British Association Report*, 1873, p. 36, I have said "military books." But the authors of the books seem to be chosen so as to represent literature in general.

thrown upon Felkel's and other contemporary factor tables by the contents of vol. v. of Lambert's *Briefwechsel*, and the part of the history of the subject, disclosed by the letters, is so curious that it seems to be worth while to give here some account of the facts connected with the numerous calculations of factor tables which were being undertaken a century ago; and this seems the more desirable as the correspondence is not very intelligible of itself without some knowledge of what preceded it, and of the books referred to in it.

I now proceed, therefore, to give a brief history of Lambert's connexion with factor tables.

§ 6. Chapter I. (pp. 1—41) of vol. II. of Lambert's *Beyträge zum Gebrauche der Mathematik und deren Anwendung* (Berlin, 1770) is entitled *Theilung und Theiler der Zahlen*, and Chapter II., *Vorschlag die Theiler der Zahlen in Tabellen zu bringen* (pp. 42—53). The latter contains the description of a mode of forming a factor table; and there is, on a folding sheet, a table giving all the simple factors of numbers not divisible by 2, 3, or 5 from 1 to 10,200. In reference to this table Lambert writes (§ 15, p. 49): "Vielmehr werde ich anmerken, dass ich die Tabelle vorzüglich deswegen durch den Druck bekannt mache, dass etwann jemand durch die so geschmeidige Einrichtung derselben sich bewegen lasse, noch 9 andere, oder wenn er sich einen recht unsterblichen Namen machen will, noch 99 andere beyzufügen. Denn so würde man im letztern Fall, auf die geschmeidigste Art, die nur immer möglich ist, die Theiler jeder Zahlen haben die unter einer Million, oder unter 1020000 sind, und das wäre doch immer genug. Im erstern Fall hätte man sie bis auf 102000, und man würde immer 10mal weiter damit reichen, als mit der gegenwärtigen, die nur bis auf 10200 geht." In the same year Lambert published his *Zusätze*, referred to above (§ 5), which contains (Tab. I.) the least factor of every number not divisible by 2, 3 or 5 from 1 to 102,000, this table occupying pp. 2—69, and (Tab. VI.) a list of primes from 1 to 102,000[1]. At the end of his introduction Lambert asks journalists and authors to make this table known, and proceeds: "Denn wer nur auch künftig Lust hat, solche Tafeln zu berechnen, der wird dann immer besser seine Zeit darauf verwenden, dass was hier nur biss auf 102000 geliefert wird, lieber biss auf doppelt oder zehnfach weiter, als das bereits berechnete nochmals berechnet werde. Es

[1] In the preface to vol. III. of the *Beyträge* (1772) Lambert gives a list of 70 errata in the factor table in the *Zusätze*, and states that Wolfram had formed a table containing the least factor of every number not divisible by 2, 3, or 5, up to 300,000 on 25 folio pages. This table Wolfram had calculated in connexion with a 39-decimal table of [hyperbolic] logarithms of the first 1,229 primes, so that the logarithm of every composite number up to 300,000 could be found at once to 39 decimals.

wäre in der That erwünscht, wenn wir von 1 bis auf 1000000 und noch weiter die Theiler der Zahlen durch blosses Aufschlagen einer Tafel haben könnten." And he then adds that to a man so unwearied and resolute (einem so unverdrossenen wackern Mann) he has in his *Beyträge* promised, as far as depends upon him (Lambert), the same immortality that Napier, Briggs, Vlacq, Justus Byrgius, Rheticus, Pitiscus, Gardiner, and Sherwin have obtained by their tables.

These earnest appeals had, as we shall now see, the double effect of inducing several calculators to apply themselves to the formation of extended factor tables, and of causing Lambert to be the nucleus of a considerable correspondence relating to them.

With regard to the tables contained in the *Beyträge* and *Zusätze*, it seems strange that Lambert should urge the extension of the table in the *Beyträge* to 102,000, when this had been done long before by Pell and Krüger, and should publish this extension himself in the same year in the *Zusätze.* But this is explained by Lambert himself in the Introduction to the latter work as follows. He copied the *Beyträge* table from Poetius, altering the form only, and at that time he knew nothing about Pell's table beyond what is stated by Poetius. Lagrange, however (to whom Lambert had sent some copies of the *Beyträge* table), found that Pell's table extended to 100,000, and then he and Lambert became acquainted with Krüger's and other tables, and the latter printed Krüger's (or Jäger's) table to 102,000 in the *Zusätze.* He remarks that Anjema's table is the only one that forms a separate work, and that we should scarcely look for a factor table in Krüger's Algebra or Poetius's Arithmetic. In several cases, he knows that the factor table has been torn out from Krüger's Algebra and retained, the rest of the book being thrown away, and he himself bought such a copy at the sale of König's books at the Hague in 1758.

Gauss, in his letter to Encke of December 24th, 1849 (*Werke*, t. ii, p. 446) states that in the list of primes in the *Zusätze* the chiliad 101,000—102,000 'swarms with errors'. Six of the errata given in vol. III. of the *Beyträge* (see the note on the preceding page) relate to primes in this chiliad.

§ 7. The first part of the fifth volume of *Joh. Heinrich Lamberts...deutscher gelehrter Briefwechsel. Herausgegeben von Joh. Bernoulli* (Berlin, 1785) contains 242 pp.; and nearly the whole of the contents of the part (which was issued as a separate volume) relates to the construction of factor tables. There is a correspondence between Lambert and von Stamford and Rosenthal, between Lambert and Felkel, and between Lambert and Hindenburg, and there are also explanatory additions and notes by the

editor, Bernoulli. At the date of publication of the volume (1785) Bernoulli states that von Stamford was an *Ingenieur-Hauptmann* at Potsdam, and had quite given up the pursuits that brought him in connexion with Lambert. Rosenthal was a *Berg-Commissarius* of Gotha, and had become known for his meteorological writings. Felkel had been a master in the normal school at Vienna, and before he began the construction of his factor table had printed some mathematical writings. He was then (1785) director of the Thun's poor-school in Bohemia. Hindenburg was professor at Leipzig, and is the mathematician of that name so well known in connexion with his writings on combinations and the combinatorial analysis. The contents of the volume will be most readily understood if I give a résumé of the short correspondence of eleven letters (30 pp.) between Lambert and von Stamford and Rosenthal. On May 10, 1774, von Stamford writes to Lambert from Ilfeld, near Nordhausen, to tell him that for six months he has been calculating some hyperbolic logarithms to 20 decimal places, but that he learns that Lambert already is in possession of them; and he asks Lambert to suggest to him some other piece of work. On May 18 Lambert replies, and states that a friend of his at Dresden [Ludwig Oberreit, an *Ober-Finanz-Buchhalter*] had sent him a few days previously a table of the prime factors of all numbers, not divisible by 2, 3 or 5, from 1 to 72,000 and from 100,000 to 504,000. The calculator had intended to extend the table to a million, but was prevented by a change of position that deprived him of the requisite leisure. Lambert suggests that von Stamford should fill in the gap from 72,000 to 100,000, and continue the table from 504,000 to 1,000,000: such a table, he adds, would be published by itself, and would form a moderately large quarto volume[1]. Von Stamford writes on May 24 expressing his willingness to undertake the completion of the table, but asks if the portion from 72,000 to 100,000 is not contained in Lambert's own table in the *Zusätze*.

On June 7 Lambert explains that his table only gives the least factors, and also that a verification of it would be desirable. The mode of calculation (which seems to be similar to that described in the *Beyträge*) is also explained. This is not worth reproducing here, though it may be mentioned that the fact that the concluding number (504,000) of the second part is seven times the concluding number (72,000) of the first part is connected with the mode of formation.

Ten months afterwards, on April 2, 1775, von Stamford writes that he has completed the filling in of the gap, so that the table is complete to 504,000. He intends to leave his position as master at Ilfeld, and to return to his former profession of engineer.

[1] This is in fact the extent and size of Chernac's *Cribrum Arithmeticum*, (1811).

He states that he has made arrangements with Rosenthal, of Nordhausen (a baker by trade, but who has written one or two small books), to halve the remainder of the work, von Stamford undertaking the portion from 504,000 to 750,000, and Rosenthal the remainder from 750,000 to 1,000,000. The next two letters, von Stamford to Lambert, July 23, 1775, and Lambert to von Stamford, August 12, relate chiefly to a method of Kästner's for computing logarithms, and but slight mention is made of the factor table. In a letter without date, but written towards the end of February, 1776, Lambert, not knowing the address of von Stamford, writes to Rosenthal to ask him the state of the work, as he has unexpectedly received from Felkel at Vienna an announcement relating to a machine for readily finding factors, and to a factor table from 1 to 144,000. Lambert states that Felkel intends to extend his table to 1,000,000, and that he (Lambert) has written to tell him of the work in progress; he hopes that Felkel will begin at 1,000,000 and continue the table from 1,000,000 to 2,000,000, and that the whole two millions may appear together. This he has proposed to Felkel, who appears to agree to it. Rosenthal's reply is dated March 6. He says that on February 2 von Stamford wrote to him to say that on account of his health he could not complete his part of the factor table, and asking Rosenthal to undertake the whole of the remainder. Rosenthal had replied in the affirmative on February 14. As for his own work (which he describes as 725,000 to 1,000,000) he is so far advanced that he could, with the aid of a factor table to 143,000, complete it in two months: he hopes that Felkel will agree to a joint publication.

The remaining two letters are from Lambert to Rosenthal. The first (July 30, 1776) shows considerable irritation against Felkel. Lambert says that Felkel has only accepted his suggestion as far as serves his own purpose. That he can find in him no trace of confidence, modesty, or fairness; that he asks him to approve of a work that he has not seen, and to obtain subscribers, when the whole affair may be only boasting. He tells Rosenthal it is useless for him to extend the table further at present. In a note to this letter Bernoulli adds that a friend of his who had formed an unfavourable impression of Felkel from his publications, changed his opinion when he knew him personally, and that very likely Lambert would have done the same. The second letter (August 13, 1776) announces a new event, viz. that Hindenburg, of Leipzig, having seen an advertisement of Felkel's undertaking, had announced a factor table to five millions, which however was only to give the least factor. Lambert suggests that it would be better if the one would begin where the other left off, and wonders whether any more factor tables will be an-

nounced[1]. He concludes with the words "Die Sache dürfte künftig in der Geschichte der Mathematik einen ziemlichen Raum einnehmen." This closes the correspondence with Rosenthal, and there is no further information about Oberreit's table and its continuation in this volume; but in the first volume of the *Briefwechsel* (no date, but preface dated 1781) Bernoulli states that at that time Oberreit's manuscript was in the hands of Professor Schulze, of Berlin. This is the Schulze who published in 1778 the well known 'Sammlung logarithmischer, trigonometrischer und anderer...Tafeln.' In his letter to Dase, printed in the introduction to the seventh million (§ 17), Gauss speaks of 'Rosenthal's manuscript to 750,000 which was in Kästner's hands.' The full sentence is, "So hatte z. B. Oberreit die Tafel bis 500000 berechnet und in Lambert's Hände gelegt; Rosenthal eine bis 750000, die in Kästner's Händen war." But, as we have seen, Rosenthal's work was only a continuation of Oberreit's, and extended from 725,000 or 750,000 to 1,000,000.

§ 8. The correspondence with Felkel consists of 13 letters, and there is an appendix by the editor, the whole occupying pp. 33—134. All Felkel's letters relate either to his table, which has been described in § 4, or to his machine, and are written from Vienna. His first letter is dated January 15, 1776. He states that he is induced to write to Lambert by the contents of §§ 15, 16 of the *Beyträge*, and that he has found a method of exhibiting all the [prime] factors of the numbers not divisible by 2, 3 or 5 from 1 to 144,000 on 12 ordinary sheets of paper, so that any one can see the factors at a glance[2]. He also mentions his machine, made of rods, by means of which he believes that the table could be continued to 1,000,000 in the space of a year. There is also added a draft announcement or advertisement, for obtaining subscribers for the table from 1 to 144,000, in which it is stated that if the table be favourably received the continuation to 1,000,000 will be published within a year. He submits this to Lambert and asks for his support. After this request has been made a second time, Lambert replies as we know, suggesting that as the first million is being calculated by

[1] The whole sentence is worth quoting: "Diese Herren wollen, wie es scheint, einander zuvorkommen, anstatt dass unstreitig besser wäre, wenn der eine da anfienge, wo der andere aufhöret. Der eine rühmt seine Maschine, der andere seine Methode. Die Zeit muss lehren, was an beiden ist, und was dann ferner zu thun seyn wird. Noch ist die Frage, ob nicht noch mehrere mit solchen Tafeln hervorrücken werden. Die Sache dürfte künftig in der Geschichte der Mathematik einen ziemlichen Raum einnehmen." (*Briefwechsel*, v. p. 30.)

[2] The words are "dass man solche mit dem ersten Blicke finden kann." It will be seen from the description given of the mode of using the table in § 4 that this is rather an exaggeration. The 12 sheets (Bogen) are the 24 folio pp. forming the principal table.

others, Felkel should undertake the second million. To this Felkel appears to consent in his letter of February 21, 1776. Lambert replies in a friendly letter and says that he will write to his two correspondents [von Stamford and Rosenthal] to learn what progress they have made. There are then three letters from Felkel dated March 30, April 24, and June 24, 1776. In the first he points out the excellence of his own process, and withdraws his consent to combine his work with that of Lambert's other correspondents: he hopes by Easter, 1777, to have the table printed up to 1,008,000, on 42 sheets, and the portion from 1,008,000 to 2,016,000 ready for press. In the second letter he states that the emperor has approved the whole work, and has made a money advance (Vorschuss) towards the printing. The third letter contains a circular (in Latin) inviting subscriptions: it is there stated that part I. (1—144,000) had been promised in February for one florin, but that it was in contemplation to extend part I. to 300,000 and publish it in August for two florins. The proposed extent of the whole table is not mentioned, though it seems to be implied that the factors have been found of numbers up to 2,000,000. The machine is also advertised for sale; the larger ones (for finding factors of all numbers) at three florins, and the smaller ones (for finding factors of numbers not divisible by 2, 3 or 5) for two florins. The circular is dated June 10, 1776.

It is after the receipt of these three letters and before writing in reply to Felkel that Lambert wrote the letter of July 30 to Rosenthal, the substance of which has been given in the last section. The wording is certainly strong: he writes, "dass Herr Felkel meinen Antrag nur so weit angenommen, als er in seinen Kram dienet... Ich finde nicht die geringste Spur von Zutrauen, Bescheidenheit und Billigkeit darinn." He mentions that he has received three letters from Felkel to which he had not replied. Lambert certainly had some grounds for the opinion he expresses, as Felkel though he says a good deal about the excellences of his machine and table does not explain them; and a great part of his communications is occupied by boasting in one form or another.

On August 13, 1776, he writes to Felkel and sends him an announcement of Hindenburg's table from 1 to 5,000,000 that he has just received. He says that he would write and ask Hindenburg to begin at the third million, only that his former proposition (to Felkel, to begin at the second million) was unsuccessful, and this might be so too. On September 10 Felkel sends to Lambert a *Nachricht* occupying 20 pp. and entitled 'Nachricht von einer Tafel, welche alle Factoren aller Zahlen von 1 bis 1 Million, dann einer andern, welche alle Factoren, aller durch 2, 3, 5 nicht theilbaren Zahlen enthält, von 1 bis 10 Millionen, als

ein Vorbericht zur ausführlichen Beschreibung der neuen Factorentafeln und ihrer Berechnungsart.' This is not a draft of that which actually appears as an introduction to Felkel's Table, which is very different. I have quoted the title in full, because it contains the first formal[1] mention of the extension of the table to 10 millions. The date at the end of the *Nachricht* is June 1, 1776, but as Bernoulli remarks in a note, this is extraordinary, as the date of the letter is September 10, and Felkel did not allude to it in his letter of June 24.

It seemed to me likely that the extension of the limit to 10 millions both in this *Nachricht* and also in the title of the work[2] itself was due to the announcement of Hindenburg's table, which was to extend to 5 millions; and that if the latter had not appeared Felkel would have fixed 2 millions as the limit or would have fixed no limit at all. But this is not the case, for in a letter of August 13, the day on which Lambert sent Hindenburg's circular to Felkel, he also writes to Hindenburg and says that Felkel in a recently printed circular has spoken of 10 millions[3]. In a note, the words (added on the margin of a Latin circular of date June 10, 1776) are given as 'Tabulæ continuantur usque ad 10 Milliones; et præterea primæ Millionis omnes factores singillatim exponuntur.' It thus appears that the proposed extension to 10 millions was not suggested by a desire to surpass Hindenburg. Felkel was very enthusiastic, and infatuated with his system of calculation; he continually speaks of the 'natural beauty of his plan,' &c. In a letter to Hindenburg of October 5, 1776, Lambert remarks that Felkel is always printing circulars instead of the table itself, from which he infers that Felkel does not receive so much support at Vienna as he expected; and in a letter of September 27 Hindenburg says that it is certain that Felkel has safely received the announcement of his table, as his own words occur in Felkel's new *Nachricht*. It seems clear from the correspondence that when

[1] In this very first letter to Lambert (January 15, 1776), Felkel, after saying that by the aid of his machine he can extend the table to a million within a year, proceeds: "Und da die Zeit noch mehrere Vortheile anbieten dürfte; so will ich die Hofnung nicht aufgeben, solche innerhalb mehreren Jahren auch bis 10 Millionen darzustellen" (*Briefwechsel*, v. p. 37).

[2] The fact that Felkel's title-page describes his table as extending to 10 millions has often led to mistakes, as it has been supposed by writers who have not seen the book to actually reach this extent; and Murhard even, by a misprint, no doubt, speaks of the table as extending to 100,000,000.

[3] The words are worth quoting: "Ich erwarte hierauf keine andere Antwort, als dass Herr Felkel selbst dieser Dritte seyn werde. Denn in einer neuen gedrukten Nachricht hat er bereits von 10 Millionen gesprochen. Er ist so voller Triumph, dass die Ersparung von Zeit und unnöthiger Mühe gar keinen Eindruck auf ihn macht, und dass er sich auch nicht besinnt, dass eigentlich mehr seine Methode und Maschine, als ein unnöthiger Gebrauch derselben ihn berühmt machen können" (*Briefwechsel*, v. p. 152).

Felkel published the first part of his table he had not calculated more than two millions. See also §§ 10 and 11.

In November, 1776, Felkel sends Lambert the first part (1—144,000) of his work, which has been described in § 4; and also some errata in it. Lambert replies on December 12, thanking Felkel for the table: he considers the arrangement in eight columns excellent, and the combination of letters and their use ingenious; but, he points out, the only object of the employment of letters is to save room, and he finds that the actual factors might have been written in figures without more space being occupied. He recognises also that Felkel's table could not well have been combined with any other table. Mention is then made of Felkel's machine (which is frequently referred to in the correspondence): it is a very simple contrivance, consisting of rods of different lengths, by means of which the measurements were to be made[1]. In the same letter Lambert also states he has received from Hindenburg his book, *Beschreibung einer ganz neuen Art*...(see § 9). As this closes the correspondence between Lambert and Felkel it is convenient here to give some account of Hindenburg's method and proposed table, and to resume Felkel's history in § 10.

§ 9. The correspondence between Lambert and Hindenburg occupies pp. 137—221, and consists of 11 letters, extending from August 3, 1776, to December 22, 1776. As Hindenburg's table was never published, it is sufficient to state the contents of the correspondence very briefly. On August 3 Hindenburg sends Lambert the *Nachricht* of his proposed table; the publication of this *Nachricht* having been accelerated by Felkel's announcement. Hindenburg proposes to give only the least factor of all numbers not divisible by 2, 3 or 5 from 1 to 5,000,000, together with a list of primes between these limits.

Lambert's letter to Hindenburg on August 13 (on this day he wrote to all his factor table correspondents) is an interesting one, as he there freely gives his opinion of Felkel. A quotation from this letter has been made in a note to § 8. The correspondence relates chiefly to Felkel's machine and to Hindenburg's apparatus, which are found to be essentially the same[2]. This of course must be the case to some extent with all mechanical appliances for constructing factor tables. From what has been said in § 1 it is clear that the fundamental principle must be the same, but considerable modifications may be introduced in details, and these may be of great importance in facilitating the formation of

[1] The machine is fully explained in an appendix by Bernoulli (*Briefwechsel*, v. pp. 232—234).

[2] *Briefwechsel*, v. p. 155 (note).

the table. Hindenburg's apparatus is described in a book[1] of which the title is "Carl Friedrich Hindenburgs Beschreibung einer ganz neuen Art, nach einem bekannten Gesetze fortgehende Zahlen, durch Abzählen oder Abmessen bequem und sicher zu finden. Nebst Anwendung der Methode auf verschiedene Zahlen, besonders auf eine darnach zu fertigende Factorentafel, mit eingestreueten, die Zahlenberechnung überhaupt betreffenden Anmerkungen. Nebst fünf Beylagen und einer Kupfertafel. Leipzig, bey Siegfried Lebrecht Crusius. 1776." It consists of 6 + 120 pp. and 9 diagram or folding sheets, containing drawings of the apparatus and specimen tables[2]. The apparatus, as drawn, does not seem to resemble Felkel's in appearance, being more complicated, the bars used having squares cut out from them; but I have not read the book. Hindenburg intended that his table should be of quarto size, similar to the Avignon edition of Gardiner's logarithmic tables; but afterwards, when he determined to include all the factors, in accordance with Lambert's wish, the title as advertised in the *Leipziger Messverzeichniss* ran "Factorentafel, in welcher alle Primzahlen und alle einfache Theiler der zusammengesetzten, durch 2, 3, 5 nicht theilbaren Zahlen von 1—1000000 in bequemen, auf der Stelle verständlichen Zeichen, ausgesetzt, befindlich sind; auf 100 grossen Folioseiten, nebst einer Einleitung. Herausgegeben von C. F. Hindenburg. Leipzig, bey S. C. (*sic*) Crusius" (*Briefwechsel*[3], v. pp. 177—178). In an appendix Bernoulli gives the following account of the causes that led to the non-publication of the table. The first delay was caused by Lambert's desire that all factors should be included; Lambert died; other difficulties occurred, some being due to the expense of publication; at length, at the beginning of 1782, all was going well, but more difficulties arose, although the manuscript of the first million, containing all the (prime) factors for each number, with the list of primes, was quite complete. When at Leipzig in September, 1784, Bernoulli himself saw the manuscript and the beginning of the table printed very beautifully and clearly on large royal folio sheets. "I must confess," he proceeds, "the whole arrangement for finding the factors of composite numbers pleased me very much; it is very much simpler than I expected. I convinced myself of this by several trials; very quickly and at a glance one can resolve into figures the symbols of the factors

[1] This is the work that Lambert tells Felkel he has received in his letter of December 12, 1776. See end of § 8.

[2] I may mention that the meaning of the anagram on the last page (p. 120) of this work is explained in pp. 176, 177 of vol. v. of the *Briefwechsel*.

[3] In vol. I. (1781) of the *Briefwechsel*, pp. 367, 368, Bernoulli stated that the first one or two millions of Hindenburg's calculation might soon be expected to appear.

on the spot, without turning over a leaf." This was written by Bernoulli in 1785. Lambert died on September 25, 1777.

With regard to the invention of a mechanical arrangement for the determination of factors, Bernoulli considers that the priority is undoubtedly due to Hindenburg, and not to Felkel (*Briefwechsel*, v. pp. 239—241).

§ 10. To return to Felkel's table: as we know, the printing was continued up to 408,000 and then, according to Gauss's statement, it was stopped, and the whole edition was destroyed, except a few copies, of which the Göttingen library possesses one, extending to 336,000. The Graves copy, however, extends to the full limit of 408,000. I was anxious to find out the cause of the stoppage of the printing and of the destruction of the copies, and I now proceed to give an account of all that bears upon this matter.

In an appendix, added by Bernoulli to the Lambert-Felkel correspondence (*Briefwechsel*, v. pp. 121—134), he states that Felkel seems to have entirely laid aside the subject of factor tables until last autumn (*i.e.* the autumn of 1784), when part of the correspondence was printed, and this revived his interest in the matter. He made a journey to Leipzig, Berlin (from which Bernoulli was absent at the time, and so did not see him), and Halle, in order to obtain support and find a bookseller. He issued a new circular, printed at Halle, which Bernoulli reproduces, and as the first paragraph contains Felkel's own account of the printing of his first factor table, and is also characteristic of his style, I give it here, together with the title:

"*Nachricht von den wiederauflebenden Felkelschen Factorentafeln; den Abdruck der ersten Million u. f. und die Fortsetzung der Berechnung* (*von* 2016001 *an*) *bis* 10 *Millionen, betreffend.*

"Schon seit acht Jahren, da dieses ungeheure, von Kennern theils bewunderte, theils bezweifelte Werk, innerhalb 18 Monaten, aus einem Nichts, bis über die Grenzen zwoer Millionen erwuchs, dessen Abdruck aber durch Kriegstroublen, Todesfälle, und andere Schicksale gesperrt, mit 408000 ausgesetzt, und gleichsam ein Opfer der Zernichtung werden musste; hat es an Gelegenheit nie gemangelt, das Urtheil der Kenner darüber weit und breit einzuholen, und sich von dessen günstiger Aufnahme zu versichern; wie nicht minder sich alle Wünsche und Ausstellungen von Wichtigkeit zu Nutze zu machen."

He here, in fact, merely says that the calculation had reached two millions, and that through misfortunes the printing was stopped at 408,000; but he does not explain why it was "a sacrifice to annihilation." The meaning of these words will, however, appear in the next section. I should state that in these *wiederauflebende*

*Tafeln*, Felkel proposed to give the least factor in figures, in accordance with the advice of Lambert (*Briefwechsel*, v. p. 241).

§ 11. In 1798 Felkel edited at Lisbon a Latin edition of the *Zusätze*, under the title *Supplementa Tabularum* (see § 5). In the *Præfatio Interpretis* he states that the *Zusätze*, though one of Lambert's best works, has not obtained the publicity it deserves, through being written in German, and accordingly the Royal Academy of Lisbon have commissioned him to make a Latin translation[1].

Felkel then proceeds to give an account of himself and his works. He did not commence the study of mathematics till he was 35 years of age, and a difficulty in 'remembering and imitating' removed from him all hope of attacking the higher branches of the science. By reading Euler's Algebra of 1768, in which the difficulty of resolving large numbers into their factors is pointed out, he was led to consider the subject of factor tables; and he proceeds: "Extemplo missis libris tardisque Scholasticorum viis[2] ad Tabulas Factorum consuetudine majores construendas me composui, confecto paucis diebus instrumento, cujus ope me victorem omnium difficultatum et Computatorum quotcunque æmulantium fore non dubitavi; basimque impressam pro aliquot millionibus procuravi. Accidit hoc Octobri 1775, tempore scilicet feriarum." He then mentions that he was confirmed in his intention by the *Zusätze*, and entered into correspondence with Lambert. The *instrumentum* is of course the machine[3], and the *basis impressa* was, I suppose, a blank form corresponding to a

[1] Felkel prefixes a *Præfatio Interpretis*, and he adds at the end an account of his labours and researches, and what he had proposed to accomplish. He also appends many notes to Lambert's introduction. Although the introduction to the *Zusätze* of 1770 is in German, the headings of the tables are in Latin: and till I saw the book I was never able to understand why the factor table should be generally quoted as "Tabula numerorum, &c.," as from Felkel's statement it seemed that the work was in German. On the first page of the tables, after the German introduction, there occurs the title *I. H. Lambert Supplementa Tabularum logarithmicarum et trigonometricarum*, and this explains the fact that the work is sometimes called the *Supplementa Tabularum* in writings between 1770 and 1798, when evidently the *Zusätze* is referred to. As regards the factor table Lambert only gave the least factor in the *Zusätze*, but in the *Supplementa* of 1798 all prime factors except the greatest are given, the least factor being printed in figures and the other factors denoted by single letters. The powers of the factors are also shown: *ex. gr.* for 24,563 we have $7f^2$ given, $f$ denoting 11. Felkel thus carries out here on a small scale the proposed arrangement for the *wiederauflebende Tafeln*, viz. to give the least factor in figures, and the others by means of letters.

[2] These words recall the subject of the engraving on the title-page of Felkel's factor table.

[3] In the *British Association Report* I said of Felkel that I had "seen (in some work of reference) a number of mathematical contrivances assigned to him as their inventor." This no doubt must refer to his machine, which was made of different sizes, &c.

page of the printed table[1]. The next three paragraphs, as they are important, and as also they are obscure in places, I quote in full:

"Quo in negotio ne conamina aliorum unita, aut deliberationes jam superfluæ moram aut præventionem causarent, oppressi omnes, absolutis una manu duabus millionibus spatio 16 mensium, expositisque præter consuetudinem aliorum omnibus cujusvis numeri Factoribus; subministrante pro typo sine mora incepto impensas Imperatore Josepho II.

"Effectus supra omnium fidem fuit, doloque extraneorum centro eruditionis propiorum prævalente desistendum a typo ad 408000 progresso, reconditis Tabulis in ærario Principio.

"Cognita tandem methodi meæ præstantia anno 1784 a diversis Academicis extraneis præprimis Berolinensibus, concurrentibusque in partem operis reformati (nomine: *Tabulæ Factorum redidivæ*) nec recepto primo Originali ad hunc finem necessario, anno 1785 computavi pro secunda vice, spatio temporis angustiore Tabulas Factorum usque ad septuplum impressarum, seu ad limitem 2,856.000. Et hoc intuitu certum est me prope 5 Millionum computatarum auctorem extitisse."

Here Felkel says that the two millions required 16 months to calculate, while in the circular upon the *wideraufiebende Tafeln* he said the "work grew from nothing to over two millions within 18 months." It does not seem to be clear to what 'oppressi omnes' refers, nor how the "craft of foreigners nearer to the centre of erudition" caused the cessation of the printing at 408,000, nor why the tables were retained in the Treasury.

Felkel's method no doubt became known to the Berlin mathematicians in 1784 through Bernoulli's researches in connexion with the publication of Lambert's correspondence.

It will be seen that Felkel here distinctly states that in 1775—1776 he had continued the table to two millions, and this is in complete agreement with what is stated in the correspondence with Lambert and in the *Nachricht von den wiederauflebenden Factorentafeln*. Also in 1785 he calculated the table *de novo* from 1 to 2,856,000, this latter number being as he states seven times 408,000, so that there is no possibility of a misprint. The two millions added to 2,856,000 give nearly 5 millions, and this is, without any doubt, what Felkel means when he says "prope 5 millionum computatarum auctorem extitisse." But in a note to p. xiv. of the *Introductio* he states distinctly that in 1785 he had calculated the table from 1 to 5 millions. His words are "non solum inveni formam omnes divisores numerorum, excepto maximo, ab 1 usque 1,008.000 in spatio 42 plagularum repræ-

[1] See *Briefwechsel*, v. p. 232.

sentandi, verum etiam reipsa opus spatio 16 mensium usque ad 2,016.000 confeci, annoque 1785 (animatus applausu diversorum Academicorum) ad 5,000.000 usque continuavi."

It seems to me very unlikely that Felkel continued the table up to 5,000,000, and I think this last statement must be incorrect; possibly the 5 may be a misprint for 3, but in that case probably the word *prope* would have been added. I draw particular attention to this matter, because it would be a very remarkable circumstance if Felkel had really carried the table as far as five millions. In the *British Association Report* (p. 37) I have quoted without remark the last statement of Felkel, merely adding a reference to p. vii. of the *Præfatio Interpretis;* as I was unable to explain the discrepancy, and supposed that at all events Felkel could not have been mistaken in describing his own work as extending to 5,000,000. But I now think that the account in the *Præfatio Interpretis* must be the correct one.

To continue Felkel's account of himself; he states that he was sensible that so large a table could not be printed, so that his project of finishing the ten millions was always deferred. He considered whether he could not devise something more portable, and in 1793—1794 he constructed 15 'bases' on this number of sheets, by the aid of which numbers up to 24 millions[1] could be resolved into their factors, the least factor not exceeding 400; and he found a method of extending the table to 100 millions on 65 'bases.' Felkel also describes work that he performed in connexion with periodic decimal fractions, or rather periodic fractions to any radix *m*. He went to Lisbon in 1791.

In a notice of the *Supplementa* contained in vol. II. of Zach's *Monatliche Correspondenz* (1800), pp. 222, 223, it is stated that the translator is the same Felkel who published the great folio factor table to 144,000. The work was ready in manuscript to two millions, and printed to 408,000 at the cost of the Treasury. But as there were no purchasers, the whole edition, except a few copies, was used for cartridges in the Turkish war[2]. The manuscript which was seized had at length been given back to Felkel,

[1] In the *Pro Notitia*, at the end, he says 24,600,000.

[2] The original passage runs: "Das Werk war im Manuscript bis 2 Millionen fertig, und bis 408000 auf Kosten des k. k. Aerariums gedruckt. Weil sich aber keine Abnehmer dazu fanden, so wurde die ganze Auflage vor Ausbruch des letzten Türkenkrieges zu Infanterie-Patronen-Papier verwendet; nur wenige vollständige Exemplare wurden dem Vulcan entrissen. Der Verfasser hat das Manuscript, welches in Beschlag genommen war, aus der Kriegs-Canzley wieder zurück erhalten; gegenwärtig hält er sich in Lissabon auf, wo er einer, vor uns liegenden Lateinischen Ankündigung zu Folge, mehrere Tabellen herauszugeben gedenkt, worunter auch Factoren-Tafeln bis auf 246000000." In a review of Chernac's *Cribrum* in the *Göttingische gelehrte Anzeigen* (1812, March 23), Gauss refers to this passage, and concludes "So ging eine verdienstliche vieljährige Arbeit für das Publicum verloren" (Gauss, *Werke*, II. p. 182).

who was at Lisbon, and had issued a Latin announcement of his factor table to 24,600,000.

This is the last mention I have found of Felkel. It will be seen that 'Opfer der Zernichtung' was not too strong a description of the fate of the table. Felkel seems to have continued to issue factor table circulars to the last.

Thus of all the persons—Oberreit, von Stamford, Rosenthal, Felkel, Hindenburg—who were induced to calculate large factor tables by Lambert's promise of immortality, Felkel is the only one who has left any record of his work: and that consists only of a very curious and rare book, of which the Graves copy is perhaps almost the only one that extends as far as to 408,000.

§ 12. The fact that so many calculations should have been commenced and none completed, is due partly to the peculiar character of the work, which requires continual and persistent attention throughout, and is of such a kind that it does not permit of being laid aside and resumed after an interval, and partly to the difficulty of arranging the table in a compact form if all the factors are to be given. Both Felkel and Hindenburg employed large folio pages.

Lambert attached great importance to the table giving all the factors; and this seems to me to have been unfortunate, as the bulk of the book is thereby greatly increased. I have in § 1 expressed a strong opinion that all that is wanted is the power to readily resolve a number into factors, and this is obtained if the least factor is given. It is undoubtedly more *convenient* to have all the prime factors: but to give all the factors is a luxury, as distinguished from a necessity. Up to 10,000 or 100,000 I should consider it most desirable to have a table, giving all the prime factors with their powers, for ready reference in cases in which it might be necessary to find a number having factors of a given form, or having a certain number of factors, &c.; but the primary object of a factor table is to save the labour of dividing a number by all the primes 7, 11, 13, 17... in order to find whether it is prime, or what are its factors. As a matter of fact, Chernac's table gives all prime factors up to a million, and it is very convenient to have the complete resolutions of numbers to this extent; but *ex. gr.* I should regard a table giving the least factors for a range of two millions as more valuable than a table giving the complete resolutions for one million.

§ 13. From the list in § 3 it will be seen that Marci published a list of primes to 400,000 in 1772. Neither Lambert nor any of his correspondents knew anything of this table, nor does Gauss refer to it. I have not seen it, and all that I know of it is

contained in Chernac's description: "Adolph. Frid. Marci, vir in rationibus subducendis exercitatissimus, sive sua sponte, sive hortatu Lamberti excitatus, animum adjecit ad terminos hujusmodi tabularum ampliandos. Tabellam adornavit, in qua exhibentur numeri tantummodi (*sic*) primi in quater centenis millibus obvii. Impressa esse (*sic*) Amstelodami, typis J. Morterre in 8. anno 1772. Hoc opusculum parcius videtur innotuisse exteris, in quorum diariis nusquam ejus mentionem factam reperio." (*Cribrum*, p. ix.)

In 1797 Vega published his well-known *Tabulæ logarithmico-trigonometricæ*, of which vol. II. contains a factor table to 102,000, and a list of primes from 102,000 to 400,031[1]. Vega does not state whence these tables were derived. He merely remarks that anyone can convince himself of the accuracy of the former by comparing it with Neumann's table. Both tables have been reproduced, I believe, in all the editions of this valuable collection of tables that have been issued. Chernac gives a list of errors in the factor table and in the list of primes on the last two pages of his *Cribrum* (1811); see § 14.

§ 14. I now come to the great factor tables that have been published during the present century, viz.:

Chernac (1811). All factors to a million.

Burckhardt (1814—1817). Least factors to three millions.

Dase (1862—1865). Least factors for the seventh, eighth, and ninth millions.

The title of Chernac's work is "Cribrum arithmeticum sive, tabula continens numeros primos, a compositis segregatos.... Numeris compositis, per 2, 3, 5 non dividuis, adscripti sunt divisores simplices, non minimi tantum, sed omnino omnes. Confecit Ladislaus Chernac...Daventriæ...Anno MDCCCXI." It contains all prime factors of numbers not divisible by 2, 3, or 5, from 1 to 1,020,000, on 1,020 quarto pages. All the prime factors are given, *ex. gr.* for 828,443 we have 7. 7. 11. 29. 53, for 828,563 we have 17. 17. 47. 61, &c.; and the primes are distinguished by long black bars which are very distinct. There are 1,000 numbers on each page, *i.e.* the factors of numbers between $1000n$ and $1000\ (n+1)$ are found on page $(n+1)$. The mode of entry is simple, and the figures are clear.

The table is exceedingly accurate. Burckhardt, who examined it (see § 16), found only 38 errors; of these 26 are due to a rule having fallen out from the top of a column and been wrongly replaced at the bottom: 3 others have been produced by a similar accident. Of the remaining 9 one is a mere transposition of two factors, 3 are errors in factors such as 13 for 23, while only 5 result from real errors in the calculation.

[1] In Hülsse's edition of Vega (1840) the list of primes extends to 400,313.

In the preface Chernac speaks of the work as one not of months, but of years. There is an introduction, containing brief notices of the previous tables and writings; a list of these has been given in § 3. The notices seem to be very accurate: all the statements that I have verified I have found to be quite correct. It is very remarkable that, although the preface and introduction occupy 21 pages, Chernac does not say a word as to the way in which he calculated the table, and he only alludes in very vague terms to the mode of formation of factor tables[1].

With regard to the title of the work, Chernac prints on the back of the title-page the following explanation: "Celeber. J. Alb. Fabricius, in Biblioth. Gr. Vol. III. c. 18. scripta Eratosthenis deperdita, sed passim ab antiquis laudata recenset, quibus annumerat etiam *Κόσκινον,* Cribrum Arithmeticum, de quo hæc verba profert: 'Nec aliud quicquam est, (*cribrum arithmeticum*) quam tabella numeros impares complectens, adscriptis ad compositos numeros communibus divisoribus, ut compositi a simplicibus distinguantur, et statim constet de compositorum divisore.' Hæc apponere volui, ne quem offendat operis inscriptio. De cribro Eratosthenis plura dicam, suo loco."

At the beginning of his historical account he writes, "Inter prisci ævi mathematicos, Eratosthenes fertur viam monstrasse indirectam, ad numeros simplices a compositis secernendos. Cujus rei testes sunt Nicomachus Gerasenus, et Boëthius. Hujusmodi investigationem nominabat Eratosthenes cribrum (*Κόσκινον*). Sicut enim cribro pollinario, partes farinæ subtiliores a crassioribus secernuntur; ita ope hujus methodi, numeri simplices a compositis, tanquam per cribrum segregantur et rejiciuntur. Cribri hujus meminit etiam Jamblichus Chalcidensis, in Isagoge ad Arithmeticam Nicomachi pag. 39 et 42 ex edit. Tennulii."

It is curious that the word which seems to have been used to denote both the table itself and the general method of forming it, should be so exactly appropriate to the perforated sheets, which play so important a part in the construction of a factor table by Burckhardt's method (see § 20).

§ 15. In 1814 Burckhardt published at Paris his "Tables des diviseurs pour tous les nombres du deuxième million, ou plus exactement, depuis 1020000 à 2028000, avec les nombres premiers qui s'y trouvent....Par J.-Ch. Burckhardt...Paris...1814."

It contains the least factor of every number not divisible by

[1] His words are: "His de causis, hujusmodi tabularum conditores, in iis adornandis via incedunt non directa, et numeros primos adgrediuntur, non aperto Marte, nec a fronte, sed a tergo. Loquar planius: methodo, a vulgari discrepante, matheseos cultoribus non ignota, quærunt compositorum divisores. Quibus inventis numeri primi antea delitescentes in apertum prodeunt, quasi nudati et divisoribus destituti." (*Isagoge ad tabulam*, p. vi.)

2, 3 or 5, from 1,020,000 to 2,028,000, on 112 quarto pages[1], there being thus least factors for 9,000 numbers on each page. Omitting headings, each page contains 80 lines and, omitting the left-hand argument column, 30 columns. The first three figures of the argument are taken from the top of the page, the next two from the heading of the column, and the last two from the argument column. Each page is divided into three portions by horizontal lines, the end-figures in the argument column for the first being 01, 07,...97, for the second 01, 03,...99, and for the third 03, 09,...99. The reason for this is clear, for if we throw out the multiples of 2, 3, and 5 from 1, 2, 3,...99 we have left 1, 7,...97, if from 101, 102,...199 we have left 101, 103,...199, and if from 201, 202,...299 we have left 203, 209,...299; after which the cycle repeats.

Thus, leaving out column headings, the page contains 80 lines corresponding to the 80 values of $r$ for which $300q+r$ represents numbers not divisible by 2, 3, or 5, viz. on the first line are numbers of the form $300q+1$, on the second numbers of the form $300q+7$,... on the last numbers of the form $300q+299$. It is interesting to notice the connexion between this arrangement and that adopted by Felkel and Euler. Felkel divides each page into two half-pages, each half-page containing 8 columns corresponding to the values of $r$ when multiples of 2, 3, or 5 are thrown out from $30q+r$. If then we imagine Felkel's page widened so as to contain ten groups of eight columns instead of only two, we should have 80 columns on a page, and if the arguments be supposed to run along each line across the whole page, we obtain Burckhardt's arrangement except that lines and columns are interchanged. The improvement effected by the change is considerable, as the arguments in Burckhardt are found at once with great ease, and the troublesome double entry process in Felkel is avoided. Burckhardt's arrangement is the same as that adopted by Lambert for his table to 10,200 in the *Beyträge*, except that the table is there printed on a folding sheet, while here a quarto page contains it. The tripartite system of Lambert was followed by Vega, who however could not place the three groups upon his octavo page, but had only room for one and a half[2]. This produces an awkward dislocation, not very easy to explain briefly, but the effect of which is that, as the headings of the columns in each group are not consecutive numbers,

[1] The size is that of an ordinary quarto, though the volume is technically a folio.

[2] In Hülsse's edition of Vega (1840) two of the three groups are contained on the same page. I have referred to the irregularity of progression in this table in a paper "Remarks on logarithmic and factor tables, with special reference to Mr Drach's suggestions." *Messenger of Mathematics*, vol. III. pp. 7—12. (May, 1873.)

the entry is somewhat inconvenient. The real improvement effected by Burckhardt was to so print the table that the three groups could all appear on one page, and that therefore the end-figures of the arguments were the same for the same line on each page of the volume.

The arrangement in Burckhardt's table, by which the least factors for 9,000 numbers are given in the space of half a square foot, is so compendious and clear that it seems scarcely to admit of any further improvement.

The least factor being given, in order to find the other factors it is necessary to divide the number by the least factor; and to facilitate this division Burckhardt gives (on one page) the first nine multiples of the primes from 1 to 1,423. It is to be observed that with the aid of Chernac's table only one division will ever be required, as a single division will bring any number not exceeding seven millions within the limits of Chernac's table.

In the introduction to the million Burckhardt explains very briefly the mode of construction of the table. He refers to Hindenburg's method as described in his book (see § 9), and points out that, although it is very simple, it is open to the objection that half the work is absolutely wasted, for, in the table as printed the multiples of 3 and 5 are rejected, and it is therefore necessary to copy again the part of the work that is retained. To avoid this Burckhardt employed a form containing $77 \times 80$ squares, and a series of sieves or perforated cards; in this way he obtained his table in the exact form in which it is printed. This method will be fully described in § 20, which contains an account of the mode of formation of the factor table for the fourth million.

In 1816 Burckhardt published the continuation from 2,028,000 to 3,036,000. There is no introduction, and the million is exactly similar to the previous one.

§ 16. In the following year (1817) Burckhardt published the first million (1—1,020,000), the arrangement being the same; so that the three millions (1—3,036,000) form a handsome quarto volume of about 350 pages[1]. All the prime factors for the first million had been given by Chernac, and Burckhardt states in the preface that he had intended to finish his volume with the fourth million, but that 'reasons of every kind' obliged him to prefer the first million. He adds in a note, "Si la vente de ces trois premiers millions paraissait assez favorable au Libraire pour qu'il crût pouvoir se charger de l'impression des 4[e], 5[e] et 6[e] million, je n'aurais que peu de chose à faire pour achever le manuscrit."

[1] The three millions were also issued together in one volume, with the title-page "Table des diviseurs pour tous les nombres des 1[er], 2[e] et 3[e] million." In these copies the introduction to the second million immediately follows the preface to the first million at the beginning of the volume.

With regard to the calculation of this million Burckhardt states that the library of the Institute is in possession of a manuscript containing least factors of numbers from 1 to 1,008,000, and that he has carefully compared this table with Chernac's, adding himself the 12,000 which were wanting in the manuscript. This work was much more laborious than he expected, "soit que M. Schenmark n'eût pas dû se servir de la méthode d'Euler, soit que les cinq élèves et amis qui s'étaient joints au savant professeur de l'université de Lund pour cet ouvrage, n'y aient pas mis toute l'attention qu'il exige." When the manuscript differed from Chernac's table, and the number had a factor, it was easy to decide which was right; but when the number was prime the examination was much more troublesome, for it was not sufficient to show that the factor indicated was not right: it was necessary to prove that the number was really prime. In this way he was obliged to examine 236 numbers, between 400,000 and the end of the million. For similar disagreements met with between 1 and 400,000, he consulted Vega's tables, which always confirmed Chernac. The errors found in Chernac have been referred to in § 14.

The only other mention of Schenmark's manuscript that I have seen is contained in a note in Lambert's *Briefwechsel* (1785) by Bernoulli, who states that it was taken by Lexell from Lund to St Petersburg and laid before the Academy there[1]. It must therefore have been taken from St Petersburg to Paris, and placed in the library of the Institute between 1785 and 1811.

It is rather curious that Burckhardt, who devoted so much labour to factor tables, and to whom we owe the best mode of constructing and printing them, should have prefixed such brief and meagre introductions to his volumes.

I have not been able to find any further reference to Burckhardt's nearly complete manuscript of the fourth, fifth and sixth millions, and do not know what became of it.

Five errata in Burckhardt are given in Dase (seventh million), but of these four occur in the introduction, and are of little consequence. Only one relates to the tables, and that is due to a 19 that has slipped back in the printing. In forming the factor table for the fourth million (§ 20) my father found another error in Burckhardt's tables, viz. the number 3,026,279 is prime; but Burckhardt gives a least divisor 79.

At the end of the first million Burckhardt has half a page to spare, and this he devotes to a table of the number of figures in

[1] The sentence in full is "Dahin gehört auch Herrn Prof. Schenmarks in Lund noch ungedruckte Tafel, die, wie mir aus schriftlichen Nachrichten bekannt ist, bis auf eine Million sich erstreckt, und von H. rn Prof. Lexell bey seiner letztern Reise über Lund nach Petersburg gebracht und der Kais. Akademie der Wissensch. vorgelegt worden ist." (*Briefwechsel*, v. p. 140.)

the periods of the reciprocals of primes. This table is reprinted by Jacobi at the beginning of his *Canon Arithmeticus* (Berlin, 1839), who found it very useful in constructing the tables of which the *Canon* consists. By a 'tabula Burckhardiana' is generally meant a table of this kind, and not a factor table.

§ 17. Gauss always took the greatest interest in factor tables. In a letter to Encke, dated December 24, 1849, and printed on pp. 444—447 of the second volume of his *Werke*, he mentions the enumerations of primes that he made in 1792 or 1793 from Lambert's *Zusätze* and in 1796[1] from Vega's tables. He was much rejoiced at the appearance of Chernac's table in 1811, and he and Goldschmidt made enumerations from Chernac's and Burckhardt's tables.

After speaking of these enumerations he proceeds, "Könnten Sie nicht den jungen Dase veranlassen, dass er die Primzahlen in den folgenden Millionen aus denjenigen bei der Academie befindlichen Tafeln abzählte, die wie ich fürchte das Publicum nicht besitzen soll?" This sentence is interesting as being probably the first mention of Dase in connexion with factor tables. Zacharias Dase had great natural talent for computation, and calculated a large table of seven-decimal hyperbolic logarithms of numbers, arranged like an ordinary seven-figure table of Briggian logarithms, which was published at Vienna in 1850.

On December 9, 1850, Gauss wrote to Dase, to repeat in writing, he states, what he had previously told him by word of mouth. This letter, which forms the chief portion of the introduction to Gauss's seventh million, is interesting and valuable. Gauss first points out the difficulty of resolving large numbers into their factors, and he then gives a brief history of the previous tables[2]. He refers to the Berlin manuscript of the fourth, fifth, and sixth millions (from which he had wished Dase to make the enumerations, as mentioned in his letter to Encke) as follows: "Die wichtigste Arbeit dieser Art ist aber das Manuscript, welches die 4te, 5te und 6te Million (also von 3000000 bis 6000000) enthält und von Herrn Crelle vor mehreren Jahren der Berliner Akademie zur Verwahrung übergeben ist. Gegen Untergang ist es also geschützt, und ich zweifle nicht, dass es über kurz oder lang auch publicirt werden wird." The next sentence, in which Gauss

[1] See pp. 49, 50, of the present volume of *Proceedings*.

[2] The account is very correct; only two points need notice, (1) Felkel's table is given as 1—336,000, and (2) in regard to Felkel's project for a table to 24,600,000 Gauss doubts if an additional cipher has not been added by a misprint. But Felkel's account shows that the number is right as printed: probably Gauss never saw the *Supplementa* itself, and only knew of the proposed table from the passage in the *Monatliche Correspondenz*, quoted in the note near the end of § 11. The mention of Rosenthal's manuscript has been referred to at the end of § 7.

urges Dase to calculate the next four millions, and expresses his opinion of the value of the tables, I also quote: "In diesem Vertrauen ist also in meinen Augen das zunächst Wünschenswerthe, dass auch die vier Millionen von 6000000 bis 10000000 bearbeitet werden, natürlich unbeschadet künftiger noch weiterer Fortsetzung, insofern Kräfte zur Ausführung vorhanden sind. Sie selbst besitzen mehrere dazu erforderliche Eigenschaften in besonderm Grade, eine ausgezeichnete Fertigkeit und Sicherheit in Handhabung der arithmetischen Operationen, und, wie Sie schon in mehreren Fällen bewährt haben, eine unverwüstliche Beharrlichkeit und Ausdauer. Sollten Sie also durch Unterstützung der begüterten, den wissenschaftlichen Bestrebungen freundlich gesinnten Bürger Ihrer Vaterstadt, oder auf andere Weise, in den Stand gesetzt werden, sich solcher Arbeit zu unterziehen, so könnte dies den Freunden der Arithmetik nur angenehm sein." He then points out that though the resolution of large numbers into their factors is less frequently required than that of smaller numbers, still this is counterbalanced by the fact that when the resolution has to be performed, without the aid of tables, the work is very much greater. With regard to the calculation, he mentions Burckhardt's method as that which should be adopted, and considers it important that the table should be printed from the calculation itself, without copying. As for the arrangement, he says, "Unbedingt halte ich hier die Einrichtung der Burckhardt'schen Tafel für die beste."

In 1862 the seventh million was published at Hamburg, the title being "Factoren-Tafeln für alle Zahlen der siebenten Million, oder genauer von 6000001 bis 7002000, mit den darin vorkommenden Primzahlen. Von Zacharias Dase. Hamburg, 1862...." The arrangement is exactly the same as in Burckhardt, there being 112 pages of tables.

In the preface it is mentioned that Dase was induced by Gauss to undertake the calculation of the tables, and then follows Gauss's letter, of which a résumé has just been given. After this it is stated that about a year before (viz. in 1860), through the support of patrons of science in Hamburg, Dase was enabled to devote himself entirely to carrying out Gauss's project. On September 11 of that year (1861) Dase died suddenly, leaving the seventh million complete, and the eighth million nearly complete. He had also determined a great part of the factors for the ninth and tenth millions. Dr Rosenberg, of Hamburg, had undertaken the continuation of the work. The preface is dated November, 1861, and is signed by the committee of the "Dase-Stiftung." There are six names, including Professor C. A. F. Peters.

The eighth million was issued in 1863; it is a continuation of the seventh million, the pages being numbered 113—224. It

extends from 7,002,001 to 8,010,000. There is a short preface by Dr Rosenberg, who edited the work.

The ninth million was published in 1865, the title page describing it as "von Zacharias Dase, und ergänzt von Dr H. Rosenberg." The page-numbers run from 225 to 334, and the table extends from 8,010,001 to 9,000,000. In a very short preface, signed "Das Comité der Dase-Stiftung," it is stated that the tenth million is near completion.

§ 18. The tenth million has not been published. In the "Bericht erstattet vom Vorstande der Mathematischen Gesellschaft in Hamburg," dated March 4, 1878, it is stated that the Dase committee were unable to publish this million for want of means. Further enquiries showed that the manuscript of the tenth million was in the possession of the widow of Dr Rosenberg. On examination it was found that the manuscript was begun by Dase, and that it had been completely finished by Rosenberg. Mrs Rosenberg was willing to give the manuscript to the society, either for publication or for presentation to a public library. It is added that the society were unable to obtain the publication of the table. In a letter to myself, dated Hamburg, May 28, 1878, Dr H. Schubert informs me that a few weeks before the manuscript was presented to the Berlin Academy.

§ 19. The manuscript of the fourth, fifth, and sixth millions, presented to the Berlin Academy by Crelle[1], and which Gauss refers to in his two letters, has not been printed, so that there is a gap from the third to the seventh million. Clearly it is very important that this gap should be filled up; and my father, feeling the improbability of the publication of the Berlin manuscript after so long a time, commenced the preliminary work necessary for the calculation of the fourth million at the beginning of last year. In reply to a letter from Professor Cayley, asking if there was any chance of the publication of the manuscript, Professor Kummer, the secretary of the mathematical section of the Academy, replied, in a letter dated April 29, 1877, that the manuscript[2] had been examined on a former occasion and found to be so inaccurate that "the Academy was convinced that the publication would never be advisable." Under these circumstances my father, with the assistance of two computers, at once commenced the actual calculation, which has been steadily continued without interruption. At the time of writing this (June, 1878) the fourth million is completely finished and ready for press; and all the numbers corresponding to least factors between 211 and 523, for the fifth

[1] Gauss only states that the manuscript was presented by Crelle to the Academy, but Prof. Kummer speaks of the tables as calculated by Crelle.

[2] More strictly the portion from 3,000,000 to 4,039,500.

and sixth millions, have been calculated and are ready for entry on the sheets.

I now give a description of the method (Burckhardt's) adopted in the formation of the tables.

§ 20. A form was lithographed, having 78 vertical lines and 81 horizontal lines (besides several other lines used for headings, &c.); it is thus divided into $77 \times 80$ oblong spaces, which may for convenience be called squares. The eighty rows are numbered, at the extreme left of the sheet, 01,07,...97; 01,03,...99; 03,09,...99; there being three horizontal broad lines separating the hundreds[1]. This is the same as in Burckhardt's tables (see § 15), each column representing 300 numbers. The advantage of having 77 columns is that the 7's and 11's are *lithographed* on the form and have not to be determined and inserted by hand. Thus if 77 consecutive columns of Burckhardt's tables be taken, and all the headings and tabular results except 7's and 11's be supposed to be removed, we have a representation of the form. The form actually used was constructed to begin from 3,000,000, so that for the exact representation of it we are to commence with the column headed 201 on p. 3 of Burckhardt's table (*i. e.* the 68th column).

Since each sheet corresponds to $77 \times 300$ numbers, a million occupies about $43\frac{1}{4}$ sheets, and as on each sheet the number of 7's lithographed is 880, and the number of 11's is 480, it follows that, by adopting a form which permits the 7's and 11's to be lithographed, about 59,000 entries are saved in each million; and, what is even more important, the accuracy of these 59,000 tabular results is assured.

The squares to which the least factor 13 belongs were obtained as follows: Find the numbers between 3,000,000 and $3{,}000{,}000 + 13 \times 300$, which are divisible by 13, but not by 2, 3 or 5. Take 13 consecutive columns of any blank form and cut them off from the rest of the form; then, supposing the first column to correspond to the column headed 3,000,000, make a mark in the squares that correspond to the multiples of 13, previously found, and cut out the squares so marked. We thus have a group of 13 columns, from which a number of squares (80) have been removed, and which may be called a screen or sieve. Place the sieve over the first 13 columns of the first sheet of the fourth million; then either empty squares or squares containing a 7 or 11 will appear through the holes of the sieve; in each

[1] The form is 31·69 in. long and 16·20 in. wide, exclusive of margins and the exterior argument numbers at the left. A somewhat smaller form would have sufficed; but this gives ample space in each square for four figures, and has not been found to be inconveniently large in use.

empty square write the number 13. Then place the sieve over the next 13 columns and proceed as before, and so on throughout the whole 44 sheets.

The sieve for the next prime, 17, contains 17 columns, and is made in the same way, viz. by cutting out the squares corresponding to the numbers between 3,000,000 and $3{,}000{,}000 + 17 \times 300$, which are divisible by 17, and not by 2, 3, or 5. Then this sieve is placed over the first 17 columns, and 17 entered in all the empty squares, then placed over the next 17, &c., and so on.

In general the sieve for the prime $p$ contains $p$ columns, and it is to be noted that every sieve, whatever its length, has exactly 80 squares cut out, one in each line. To show that there must be one square cut out in each line it is only necessary to observe that $p$ must have some multiple, not divisible by 2, 3, or 5, of the form $300q + a$, where $a$ is any one of the 80 numbers less than 300 and prime to it. For, by a known theorem, if $p$ be prime to $r$, and if $p$, $2p$, $3p$,...$(r-1)p$ be divided by $r$, the remainders are the $r-1$ numbers $1, 2, 3, \ldots r-1$; in this case, therefore, if $p$, $2p$, $3p \ldots 299p$ be divided by 300, the remainders are the 299 numbers $1, 2, 3, \ldots 299$, and if $2p$, $3p$, $4p$,... and all the multiples of $p$ divisible by 2, 3, or 5 be thrown out, the remainders divisible by 2, 3, or 5 are thrown out also, and the remainders left are the 80 numbers less than 300 and prime to it. Also, there cannot be two squares in the same line cut out from the sieve, for $a$ being a given number, if $300q + a$ be divisible by $p$, the next number in the same line divisible by $p$ is $300qp + a$, viz. is a number $p$ columns further on.

The cube root of 4,000,000 is 158·74..., so that the prime 157 appears once, and only once, as the least factor of a three-factor number, viz. for 3,869,893. Thus 163 and larger primes will only occur as least factors of two-factor numbers, and we may find the numbers to which they belong without the use of the sieves as follows:

Supposing that we are constructing a factor table from the commencement, the least factor 163 first appears at the number $163 \times 163$, then at $167 \times 163$, $173 \times 163$, $179 \times 163$, $181 \times 163$, &c.; 163, 167, 173, 179, 181, &c. being the series of primes starting from 163; for we only consider products of two primes, of which 163 is the smaller, that is, numbers formed by multiplying 163 by the primes greater than itself. To obtain the results of the multiplications it is only necessary to add to $163 \times 163$ the product $4 \times 163$, and to this $6 \times 163$, &c.; the work standing thus

$$\begin{array}{rcr} 26{,}569 &=& 163 \times 163 \\ 652 &=& 4 \times 163 \\ \hline 27{,}221 &=& 167 \times 163 \\ 978 &=& 6 \times 163 \\ \hline 28{,}199 &=& 173 \times 163 \\ 978 &=& 6 \times 163 \\ \hline 29{,}177 &=& 179 \times 163 \\ 326 &=& 2 \times 163 \\ \hline 29{,}503 &=& 181 \times 163 \\ \&\text{c.} && \&\text{c.} \end{array}$$

This process will give all the numbers to which 163 belongs as least factor up to $(163)^3 = 4{,}330{,}747$, where the three-factor numbers commence. All that is required in order to reduce this to mere addition is a list of differences of consecutive primes from 163 to $\frac{1}{163} l$, $l$ being the limit of the table, supposed less than 4,330,747, and a small table of even multiples of 163 from $2 \times 163$ to $2m \times 163$, $2m$ being the greatest difference between two consecutive primes between these limits. If $l$ be 4,000,000, the nearest prime below $\frac{1}{163} l$ is 24,533; and the greatest difference is 52, between 19,609 and 19,661[1]. The accuracy of the work can be verified at any stage and as often as thought necessary by multiplying together the two factors. Of course in the calculation of the fourth million the commencement would be made at $18{,}413 \times 163 = 3{,}001{,}319$, the smallest number exceeding 3,000,000 to which the least factor 163 belongs.

We thus have two distinct methods, each of which has its special advantages, viz. the sieve method and the method by calculation of multiples. The latter is unsuitable for small primes, which appear as least factors of numbers having three or more prime factors; in fact this method is only appropriate for two-factor numbers. On the other hand, the sieve method is rather more suitable for the entry of small primes, as when the prime is large, the great size of the sieve is inconvenient; this method points out *all* multiples of the prime, not divisible by 2, 3, or 5, whether they be two-factor, three-factor, four-factor, &c., numbers.

It is clear that up to 163 the sieve method should be used; and that for 163 and beyond we may employ the multiple method. Burckhardt states that he used the sieves for primes up to 500, and the multiple method for higher primes. In the calculation of the fourth million my father used sieves for primes up to and

[1] The greatest difference between two consecutive primes up to 100,000 is 72 (31,397—31,469). For a list of the differences that exceed 50 and other allied tables, see *Messenger of Mathematics*, vol. VII. pp. 174—175 (March, 1878).

including 307, and the multiple method for primes from 211 to 1,999. The numbers corresponding to the least factors from 211 to 307 inclusive were obtained by both methods.

As the multiple method only gives numbers where the least factor is the given prime $p$, it follows that every number so found must correspond to an empty square, and the verification thus afforded of the entries already made was very valuable.

The sieve for 307 contains 307 columns, and therefore occupies 4 sheets all but 1 column: considered as a whole, therefore, it has only to be moved 11 times for the million, while the sieve for 13 has to be moved 257 times[1].

Before the calculation was begun, it seemed as if the excessive length of the sieves (the 307-sieve would be 10 feet 6 inches in length, and the 499-sieve 17 feet 1 inch) would be productive of great inconvenience, and would also necessitate very great accuracy and care in the lithographing and printing of the sheets, so that the squares should correspond exactly, over so great a distance; and it seemed surprising that Burckhardt should have continued the sieve method so far. But this was on the supposition that the portions of the sieve would be all fixed together, so that it would consist of one long sheet. Experience, however, soon showed that nothing was gained by fixing the sheets together, and in fact that it was a positive inconvenience to do so. The sheets forming the sieve were numbered 1, 2, 3, &c., and all that was requisite was to use sheet 1 first, then sheet 2, then sheet 3, then sheet 1 again (if the sieve consisted of only 3 sheets), and so on: in fact, the long sieves were found to be quite as easy to use as the smaller ones. Above 307, however, it seemed to be scarcely worth while to construct the sieves, as so little use was made of them, and as the multiple method was preferable in consequence of the verification afforded by it.

The sieves were formed thus: Take for example 13; the first uneven multiple of 13 exceeding 3,000,000 is 3,000,023; add 26 continually till $3{,}000{,}000 + 13 \times 300$ is reached, and then throw out the multiples of 3 and 5; there are thus left 80 numbers, which correspond to the squares to be cut out from the sieve. The accuracy of the 80 numbers that remain was verified by

[1] [In the fourth million the 13's were entered by a sieve consisting of 13 columns, the 17's by a sieve of 17 columns, and so on. In the fifth and sixth millions now in progress, the 13's are being entered by a sieve of 78 columns, equivalent to six 13-sieves fixed together. This is found to greatly facilitate the entries, as the number of removals of the sieve is reduced in the proportion of 6 to 1, and there is less risk of error. The saving of time effected by the use of the 78-column sieve amounts to nearly one-half. For the 17's a sieve of $5 \times 17$, $=85$, columns will be used, for the 19's a sieve of $4 \times 19$, $=76$, columns, and so on, the number of columns being made as nearly as possible equal to the number of columns (77) on a sheet.—July 22, 1878.]

differencing them; as the differences recur with a period of eight[1].

The mode of work was as follows: The entries were made by the sieves, and one multiple of $p$ obtained from each position of the $p$-sieve was divided out by $p$, in order to verify that the sieve was always rightly placed: this verification was employed for each position of every sieve. The numbers were then examined by my father himself by the sieves. They were then examined a third time by the sieves, and every number ticked. The least factors obtained by the multiple method were read out and entered on the sheets; and they were subsequently read out again in a different manner and ticked. Any numbers found unticked were afterwards specially examined. The proofs of the table when printed will be read with the original calculations of numbers by the multiple method.

On the whole the method of construction is a very perfect one. I have explained it in some detail, because Burckhardt contents himself with a very brief sketch occupying only two paragraphs; and the process is sufficiently interesting to deserve a more complete account. Each sieve, as stated, has 80 squares cut out, one in each line; though of course, as there are only 80 squares cut out, whatever be the length of the sieve, many of the columns on the longer sieves are left intact. The patterns formed by the holes in the sieves were very curious, some being very regular, while in others the holes were very scattered, and no two were much alike. The sieves for 149 and 151 were remarkable, the holes running steadily up in the one case and steadily down in the other[2]. The reason for this is that these numbers are nearly equal to the half of 300, the difference between two adjacent squares in the same line, so that numbers distant from one another by even multiples of 150 are in the same line. For a similar reason the holes in the sieves for 59 and 61, and 29 and 31, show a steady ascent and descent. The squares in the sieves were cut out by a punch made for the purpose.

It will be evident from this description that it would be just as easy to enter all prime factors in the table as to enter only the least; and if all the prime factors were entered the verification would be far easier, and in the numbers entered by the multiple method no error could occur, unless the same mistake were made independently in entering both factors.

The methods described in this section are no doubt practically

[1] It is easily seen that this must be so; for form the multiples of the prime $p$ that are not divisible by 2, 3, or 5; these are $p$, $7p$, $11p$, $13p$, $17p$, $19p$, $23p$, $29p$, then the next eight are obtained by adding $30p$ to each of these and so on. Thus the differences are $6p$, $4p$, $2p$, $4p$, $2p$, $4p$, $6p$, $2p$, recurring with a period of eight.

[2] Twenty-six of the sieves were exhibited to the Society when this paper was read, including these two.

identical with those employed by Burckhardt, and the calculation of the million suggested no improvements upon them, except in a few matters of detail. The construction of the table, though very simple in theory, required such continual care at every step, and such constant supervision, that it could not be undertaken by any one who was not prepared to devote a great portion of his time to the work.

§ 21 (Appendix to § 2). I have always found that the factor tables of Burckhardt and Dase afforded the best practical method of obtaining an isolated logarithm to more places of decimals than can be obtained directly from the tables. The principle of the method is best exhibited by an example. I required the hyperbolic logarithms of the first eleven Bernoullian numbers to 24 places of decimals. Of these the numerators and denominators are all composed of prime factors less than 10,009 (the limit of Wolfram's table[1]), except the numerator of the ninth Bernoullian number, which is the prime number 43,867. Now $50 \times 43{,}867 = 2{,}193{,}350$, and on looking in Burckhardt for a number near to this, which shall have no prime factor greater than 10,009, it appears that

$$2{,}193{,}349 = 23 \times 47 \times 2{,}209,$$

and thus

$$43{,}867 = \tfrac{1}{50}\,(23 \times 47 \times 2{,}029 + 1),$$

and therefore

$$\log 43{,}867 = \log 23 + \log 47 + \log 2{,}029 - \log 50$$
$$+ \frac{1}{2{,}193{,}349} - \frac{1}{2}\,\frac{1}{(2{,}193{,}349)^2} + \frac{1}{3}\,\frac{1}{(2{,}193{,}349)^3} - \&c.$$

The first term of the series in the second line

$$= 0{\cdot}0000\ \ 0045\ \ 5923\ \ 7950\ \ 7319\ \ 6286\,;$$

dividing this by $2 \times 2{,}193{,}349$ we obtain

$$0{\cdot}0000\ \ 0000\ \ 0000\ \ 1039\ \ 3325\ \ 3457,$$

and the third term is

$$0{\cdot}0000\ \ 0000\ \ 0000\ \ 0000\ \ 0003\ \ 1590,$$

so that the series

$$= 0{\cdot}0000\ \ 0045\ \ 5923\ \ 6911\ \ 3997\ \ 4419\,;$$

whence, taking out the logarithms from Wolfram's tables,

$$\text{hyp. log } 43{,}867 = 10{\cdot}6889\ \ 1760\ \ 7960\ \ 5681\ \ 0191\ \ 3661.$$

[1] Wolfram's table gives hyperbolic logarithms of all numbers up to 2,200, and of primes (as well as of a great many composite numbers) up to 10,009, to 48 decimal-places. It first appeared in Schulze's *Sammlung* (1778), and was reprinted in Vega's *Thesaurus* (1794).

The principle of the method is to multiply the given prime (supposed to consist of 4, 5 or 6 figures) by such a factor that the product may be a number within the range of the factor tables, and such that when increased by 1 or 2 the prime factors may be all within the range of the logarithmic tables. The logarithm is then obtained by use of the formula

$$\log(x+d) = \log x + \frac{d}{x} - \tfrac{1}{2}\frac{d^2}{x^2} + \tfrac{1}{3}\frac{d^3}{x^3} - \&c.,$$

in which of course the object is to render $\frac{d}{x}$ as small as possible.

If the number be incommensurable, or consist of more than seven figures, we can take the first seven figures of it (or multiply or divide the number by any factor, and take the first seven figures of the result), and proceed as before. An application to the hyperbolic logarithm of $\pi$ is given by Burckhardt in the Introduction to his second million (see § 15).

I have often employed this method, and even with the gap from three millions to six millions I have never found any difficulty in rapidly obtaining the logarithms I required. Of course there are methods which may be more expeditious, if the calculator is thoroughly conversant with them and accustomed to their use. But if it is only occasionally that a logarithm has to be calculated, the factor method possesses great advantages; for there is no rule to be remembered, and the reasoning is so elementary that there can be no doubt as to whether the principles have been correctly applied. Whenever a rule or method is but rarely used, great care is necessary in applying it, unless the reason for it is so self-evident that the work of itself shows that no error in principle has been committed.

In the calculation of a table that is to extend to $n$ places it is usual to calculate the values to $n+2$ places, and if the last two figures are 50, and it is desired that the last figure retained shall be always the nearest to the truth, it becomes necessary to extend the calculation in this case to $n+3$ or $n+4$ places, which probably exceeds the number of places for which the logarithmic tables are available. Recourse must then be had to some method of calculating logarithms, and it is in occasional cases of this kind or such as that mentioned above that I have found the factor method so convenient.

CONTENTS OF THE FOREGOING PAPER.

*February* 25, 1878.

PROFESSOR LIVEING, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society by DR PEARSON:

(1) "*On a new kind of self-acting weir recently introduced on the rivers of France and Belgium.*"

River navigation in France has for a long time been under the control of the government, and employs a considerable number of engineers belonging to the department of the *Ponts et Chaussées*. About the year 1835, one of them, M. Poirée, being dissatisfied with the old kind of *staunch*, then the only artificial system used upon the river Yonne, where he was employed, to facilitate the navigation, resolved on endeavouring to introduce a method which would represent modern progress in mechanics. To carry out this view he erected with the permission of the authorities at Epineau, near Auxerre, a new weir of which the following is the construction. The river at the spot was about 120 yards broad, but had only an average depth of about a foot. Having faced the bank adjoining the towing-path with masonry, he constructed a cill or apron diagonal to the course of the stream, about 70 *mètres* long and 10 *m.* broad. At the farther end of this he placed a pile of

masonry, and the portion of the river bed between this and the opposite bank he closed with a solid weir running in a slanting direction up the stream. The new weir properly so called was placed in the space of 70 *m.* designed for the passage of barges. It consisted of a framework of iron and wood, the staunchions of which were furnished with hinges at the feet, so that they could be lowered into the bed of the stream. The framework was closed against the passage of water by means of narrow paddles or staves, called *aiguilles*, having handles at the top, resting against the framework itself in a nearly vertical position. These *aiguilles* were of wood, 1½ inches square, and 6 feet long, thus giving when in their places, a depth of at least 5 feet above the weir. The framework was so constructed that a space of about 30 yards could readily be opened or closed in less than an hour: the rest of the barrier was designed to remain upright, except in case of floods [1].

About the year 1851, M. Chanoine, another engineer of the same branch of the public service, was permitted to introduce a new modification based on the plan of M. Poirée, but including a completely different idea, viz. the employment of valves, rotating on an axis under the action of the water. In M. Poirée's system, there was a double row of staunchions running across the stream, connected at the top and bottom by iron bars, with their feet moving on hinges, but falling, when lowered, in a direction across the stream. Another horizontal bar running near the top of these staunchions supported the *aiguilles*: the upper row of bars joining the staunchions also supported loose planks, forming a kind of light bridge. In M. Chanoine's scheme, the staunchions stood in pairs at intervals of about four feet, their hinges being so placed that they would fall, when lowered, in the direction of the stream: the staunchions supported valves or floodgates, about ten feet high by four broad, turning on an axis placed at a distance of not less than one third, but always less than one half of the whole length of the valve above the cill of the weir, reckoning from the lower end: and at or near such a point that if the water above and below the weir was at its normal height, the lower end of the valve would be pressed against the cill of the weir by the water on the upper side: but that if the water rose much higher, the pressure on the upper section would become greater than that on the lower, make the valve itself rotate (or "basculer," whence the inventor termed these valves *hausses à bascule*) and allow the stream to flow freely through the aperture, the valve remaining in a nearly horizontal position until the water of the river had again subsided; should there be a serious flood, the staunchions (or *chevalets*) were so constructed as to admit of being lowered into

[1] A full description of this barrier, with plates, &c., will be found in the *Annales des Ponts et Chaussées*, Vol. II. 1839.

the bed of the stream, and this was effected as follows: Each pair of staunchions supporting a valve was maintained in an upright position by means of an iron support forked at the upper end, about four inches in diameter and eight feet long, resting at an angle of about 38° to the horizon against a ledge in the apron of the weir: this ledge makes an angle of about 3° or 4° with the vertical: and a long bar with claws attached, working in a stone groove diagonal to the stream, is so placed to run under the ends of all the supports (called in French[1] *arc-boutants*) that it can lift them one after another over the ledge: they then slide down on the apron of the weir, and the valves, supports, and staunchions, all assume nearly a horizontal position. The bar rests at either end in a chamber at the side of the stream, and is worked by means of cog-wheels and a windlass. When the flood has passed away, the weir can be easily raised again by drawing the whole up-stream by means of a chain or rope attached to the foot of each valve successively, and working on a windlass placed in a boat moored above: when the end of the iron support reaches the ledge, it catches there, while the staunchions are by that time in an upright position, and the lower end of the valve, the chain on the windlass being relaxed, is pressed by the current into its original place. The plan had the ostensible advantages of permitting a rise in the river to open the weir of itself, and making the weir itself practically removeable in a few minutes: and these theoretical merits in no way disappear entirely, under the modifications which it has been found advisable to apply to the plan where carried out in practice.

As a matter of fact, the method was employed by M. Chanoine from the first in two different ways. Recognising the great obstacle that a solid weir of any kind in a river is to the discharge of a flood, he divided the weir erected by him at Conflans (near Montereau on the Upper Seine) into two parts. Besides making a lock of the usual kind adjoining the bank, he formed the river bed, as M. Poirée had done, into two sections: in one, about 40 yards broad, he constructed the apron of the weir in the bed of the stream: in the other part, separated from the first by a buttress of masonry, he placed the moveable framework on the top of a solid weir of rubble-work, diminishing of course the size of the staunchions and valves considerably: in the first section, which he designed for the passage of vessels when the river was high, he made the valves of the full length, in the other they were not more than six feet long and the staunchions and supports of course smaller in proportion. The valves also in the first portion, having their hinges at a distance of about five-twelfths of the distance

[1] *Arc-boutant* means, in architecture, a *flying-buttress:* it is also used metaphorically as e.g. *l'arc-boutant de l'état.*

from the bottom, were made so as not to rotate of themselves except in an extreme case, while in the other part, they were so balanced as to be nearly or quite self-acting. A full description of the practical working of this weir when first erected, in Sept. 1860, will be found in the *Ann. des Ponts et Chaussées* for the following year, p. 334; from which it appears that the two divisions at Conflans are respectively 40 and 28 yards in length: that in the larger section, viz. that designed for the passage of vessels, the framework could be lowered entirely in less than two minutes, and restored to its upright position in less than an hour: while the valves surmounting the solid weir, in this case only about four feet and a half in height, were found to open or close according as the water rose or fell in the river above them; in opening they were allowed to act of themselves, but having a counterpoise attached to the lower end, which moved higher up the valve when the latter tended to become horizontal, they only returned to their original position as the water fell, by the aid of a workman who crossed the weir and pushed back the counterpoises, one after another, to the lower end of the valve.

Though it is stated that at first the smaller, self-acting valves surmounting the solid weir, operated perfectly well, yet for various practical reasons they have generally been so altered, as to be raised or lowered by means of chains attaching each end of the valves to a moveable windlass running on a truck along a light bridge, corresponding to that employed in M. Poirée's system. In fact it seems that M. Chanoine's original system, as explained by him in the *Ann. des P. et C.* 1859, 1861, has partially been modified, as has been found necessary for practical success: at the same time his original theory is still retained, and his application of it in the construction of that part of the weir, which is designed to maintain rather than to regulate the level of the upper water, is found to work perfectly well.

The entire theory of the subject, as worked out by M. Chanoine, will be found in a long article in the vol. of the *Ann. des P. et C.* for 1861. In the vol. for 1873, there will be found a full description of the weir at Port à l'Anglais on the Seine, about 3 miles above Paris, immediately above the junction of the rivers Seine and Marne: this weir, originally erected about 1860, was considerably altered in 1869, and in its present form it represents the system as adapted to the requirements of a large traffic, and including the various modifications suggested by an experience of several years. Any one desirous of thoroughly understanding the subject, should refer to the papers I have already cited and others in the same series, especially one in the vol. for 1866, the plans annexed are extremely elaborate, and explain the construction down to the minutest details. Persons who wish to see the system

in work, may visit any of the weirs on the upper Seine, or those on the river Meuse between Namur and Dinant; the flow of water in these two rivers is very much the same, and I was informed by the men in charge of the weir near Corbeil on the Seine, and also of one near Dinant, that the system was working without the least difficulty, and as well as could possibly be expected.

I will next endeavour to give a short analysis of the way in which the inventor discusses the question as a hydrostatical problem. He considers separately the different forces which act on a valve upright on its staunchions and partly immersed in the water, and tend, some to make it revolve on its axis, some to prevent its doing so.

The forces tending to make it revolve are:

1. The pressure of the upper water on the upper section of the valve.
2. The pressure of the lower water on the lower section.
3. The weight of the upper section itself.
4. The *vis viva* residing in the current.

The contrary forces are:

1. The pressure of the upper water on the lower section of the valve.
2. The pressure of the lower water on a small part of the upper section.
3. The weight of the lower section.

The relation of equilibrium between all these forces is necessarily somewhat complicated; but if we observe that the principal forces arise from the pressure of the water, especially the upper water; and if also, to simplify the question, we consider the valve itself a mathematical plane, and the axis of rotation, a line drawn in this plane, we arrive at the following result:

1. If the axis of rotation is so placed that the length of the lower section of the valve is half that of the upper section, there will be equilibrium between the pressures exercised by the upper water on the two parts of the valve, supposing the water to be just covering its top.
2. If the axis of rotation be placed in the middle of the valve, there can be no equilibrium between these pressures, whatever be the height of the water flowing over the upper part of it; the pressures on the lower section will always be in excess of those acting on the upper one.
3. From this we conclude that the axis of rotation ought to be placed at a point situated between $\frac{1}{3}$ and $\frac{1}{2}$ of the height of the valve, reckoning from the bottom, for those which do not act of themselves: and at a point about $\frac{1}{3}$ from the bottom for the self-acting valves.

If we next endeavour to estimate the value of the forces arising (1) from the water on the lower side; (2) the weight of the upper section of the valve; (3) that of the lower do.; (4) the friction; (5) the *vis viva* of the current, we find that,

1. The pressure of the lower water assists in producing the rotatory motion, but this pressure is destroyed by slightly raising the height of the axis, *i.e.* an inch or two.

2, 3. It is best to weight the lower section of the valve, to balance the weight of that part of the upper section not within the water.

4. The friction on the trunnions of the axis is very small compared with the forces acting on the valve; and an increase of ·8 to 1·2 in. in the depth of the overflow is sufficient to neutralize it.

5. The effect of the *vis viva* of the current is very small when the river is not high, and the depth of the overflow not more than 4 inches, but it will increase rapidly when the stream of itself is deep enough to submerge the axis of rotation (*i.e.* is considerably above the *étiage*)[1]; and when the overflow amounts to or exceeds six inches: and generally it will increase with the rapidity of the stream, which is itself affected by secondary causes, *e.g.* by opening a part of the navigable pass. For example, it has been ascertained, from oscillations observed in the valves of the navigable pass, that the forces acting on the upper and lower sections of the valve are literally in equilibrium when the overflow approaches to six inches in depth, (the water in the river, and consequently that also below the weir, being otherwise decidedly high), but that two or three valves in the pass lost their equilibrium as soon as some adjoining ones were lowered by the men in charge, in consequence of the increased velocity of the current.

It will be found by calculation, with a valve 10 ft. high by four feet broad, that the value of this force, or *vis viva*, the current being 1 mètre (= about $2\frac{1}{4}$ miles an hour) is represented pretty nearly by a force of 176 lbs. av. on the centre of the upper section, or *volée*, of the valve, in a direction at right angles to its surface.

The valves of a navigable pass were not designed by the inventor to be actually self-acting, for floating substances will pile themselves up against the upright staunchions, and be entangled between them and the lower parts of the valves when the latter are lying flat on the apron of the weir: and so make them project

[1] *Etiage* is a word of common occurrence in the papers dealing with the subject: I believe it means the normal level at which the stream would maintain itself, if there were no weir or sluice. It is said to be derived from *æstivaticum*, i.e. the *summer level*. For example, on the Nilometer, it would be the *zero* of the scale.

above the cill in a way which will be dangerous to craft passing through.

The self-acting valves, properly so called, were only designed to be placed on the top of the solid weir adjoining the navigable pass: their principles of construction are identical with those of the valves employed in the pass, but they were left, according to their original design, to act for themselves, rotating, when the water rises above a certain level, and recovering their position when it falls below it. A description of the way in which they would act on this theory will be found in the volume of the *A. d. P. C.* for 1681; but from more recent notices, it seems to have been found better to add a light bridge carrying a windlass, similar to that employed in M. Poirée's system, so that the natural movement of the valves may be regulated by means of a chain; the whole to be partly removed, partly lowered into the bed of the stream, in case of a serious flood.

As stated before, if the axis were placed at $\frac{1}{3}$ of the distance from the lower end of the valve, it would revolve as soon as the water passed over the top: to remedy this, the height of the axis is raised a little, and the lower end of the valve had originally a counterpoise attached, moveable by the sluice-keeper or of itself in a peculiar way described in the volume last referred to: but this contrivance has now been superseded, as has been already observed.

The calculation of the point at which the valves will open of themselves seems difficult, as of course the water rises to some extent simultaneously above and below the weir; but it seems clear that equilibrium will be destroyed:

(1) by the water rising on the weir from above,

(2) and also, supposing the water to be more than 3 feet above its normal level; the axis of rotation being fixed somewhat less than 4 feet above this point.

It does not come within the scope of this paper to give any longer abstract of the mathematical investigation of the question as stated by M. Chanoine in the *Ann. des P. et C.* for 1861; but a brief description of the several parts of the weir seems desirable, and with the help of the annexed plate, there will be little difficulty in comprehending its action.

Fig. 1. 1. represents M. Poirée's design as described in the *Ann. des P. et C.* for 1839. *A* is here the abutment of masonry. *B.B'.* are two bars which support the *aiguilles*, resting on staunchions or *fermettes*, *C.C'.* moving on hinges at *C'*, and capable of being lowered by aid of them across the stream. *D.D.* are the staves or *aiguilles*. In Fig. 1. 1. the water *L* is represented as at a nearly uniform level above and below the weir; the sluice-keeper is engaged in replacing the staves, in order to close it. Fig. 1. 2. ex-

hibits a cross section, with the plank bridge $E$ resting on bars joining the two staunchions: here the water $L$ is nearly, but not quite, up to the normal height when the weir is closed. This sketch, as mentioned above, is only introduced, as representing the system of moveable *fermettes* devised by M. Poirée, from which M. Chanoine borrowed his idea of moveable *chevalets*, supporting self-acting valves.

Fig. 2. The valves, or *hausses* (Fig. 2. 1. *A.*) are of wood: M. Chanoine made them for the navigable pass, of 10 feet high, by four broad, with the axis at a distance of $\frac{5}{12}$ from the bottom, omitting the portion of the valve resting on the cill when upright: when placed upon a solid or "tumbling" weir, the axis is lowered to a point $\frac{7}{20}$ths from the bottom, thus making the valves revolve more easily; but on the other hand the valve itself is only from 4 to 6 feet high, while the breadth is a few inches greater than in the other case. The weight of a full-sized valve immersed is about 2 cwt.

The upright staunchions or *chevalets* (*B.*) are adjusted in size and dimensions to the valves which they support; the weight of each is about 1 cwt. 3 qrs.

*C.C.* are the *arc-boutants* or supports. They are inclined at an angle of about 53° to the vertical, and weigh about 2 cwt. 1 qr. each. They rest against a ledge inclined at an angle of about 3° to the vertical, called a *heurtoir*, and are easily lifted over it by means of the claws of the

*D. Barre à talons*, a long iron bar, weighing about 11 cwt. lying in a groove beneath the ends of the *arc-boutants*, and turned by a cogwheel, upright bar, and lever: the two bars working on one another in a chamber in the abutment of masonry at the end of the weir. The force exerted on the lever in working the upright bar, is about 20 lbs.

*E.E.* are the *fermettes*, or upright iron staunchions supporting $F$, a windlass running on a light tram, $G$; the windlass working the chains $H.H'$ which raise or lower the valve. *E.E.* can be lowered into the stream by means of their hinges at *K.K.* *L.L.* represents the water when the stream is at its ordinary level.

Fig. 2. 2. represents a transverse section of the valve, and abutment adjoining.

Fig. 3. represents the valve and its apparatus lowered into the stream during a flood. In December, 1872, at Port à l'Anglais, on the Seine, the water rose about 19 ft. or nearly double the height of the valves when upright, above the cill of the weir.

Fig. 4. represents, not quite accurately, the method of raising a valve and its supports, when the water in the river has fallen. The constant pressure of a full-sized *arc-boutant* against its ledge, when the stream is in its ordinary condition and the valves vertical, being estimated at about 4 tons, the force to be exerted on

the windlass, if the water be nearly level, will be about 25 lbs.; if there be a fall of about 2 ft., as will possibly be the case, if there be much stream and a large proportion of the valves are already raised, it may be doubled, but not much more. The tension on the chain in the first case will be about 84 lbs. and increase proportionately as the water rises.

I am much indebted to W. M. Fawcett, Esq., for permitting the sketches given in the annexed plate, and also those exhibited to the Society, to be drawn in his office. But much more perfect plans including the dimensions and all other details, will be found in the volumes to which I have already referred, viz.:

The *Annales des Ponts et Chaussées*, 1839, Memoires, vol. 2, p. 238,
" " " 1861, " p. 209, (for the mathematical investigation.)
" " " 1866, Memoires, p. 172,
" " " 1873, " p. 198.

I may add that though the technical terms employed are a little puzzling to one not a professional engineer, I have found no difficulty, with the aid of M. Littré's Dictionary, in interpreting them satisfactorily.

With the exception of a few remarks made by Major Trench, R.E., M.P., in the last Parliament, I think in the year 1873, in a debate on some proposed improvements in the Shannon, I do not recollect having seen any reference in any English publication to the system described in this paper: nor does it seem to have been discussed, where it might most naturally have been expected, in the *Proceedings* of the Institution of Civil Engineers. It is right to mention this, in presenting a communication on a subject with which I cannot claim to have a technical acquaintance.

P.S. *July* 18.—On examining the weir at Port à l'Anglais in June, I observed that the *chevalets* hinge within the *hausses*, or valves; and are actually beneath them, so that there is nothing intervening between each pair. Otherwise, I think my description is quite exact in all important details.

(2) "*On a manuscript Table of Napierian Log. Sines, &c.*"

Dr Pearson also exhibited to the Society a MS. volume, belonging to Emmanuel College, containing Tables of Natural and Logarithmic Sines, &c., &c. It is incomplete, and from the name written at the beginning, seems to have belonged originally to Mr Thomas Leigh (or Lee), a well-known schoolmaster at Bishop Stortford, during the latter part of the seventeenth century[1]. A complete specimen of the tables, viz., $5^{\circ}.0'$ to $5^{\circ}.5'$ and

[1] Mr Lee seems to have graduated as B.A. at Oxford, in 1641; but in 1646 he incorporated at Emmanuel, became a Fellow and took his M.A. degree. In 1663 he

30°. 0′ to 30°. 5′, is printed below. The natural sines, &c., are of course nearly identical with those now used; but the logarithms are the original logarithms actually invented by Napier, and in their form, though not otherwise, differ from the natural or hyperbolic logarithms, generally so called; and which have been published at length by Wolfram and Dase. They decrease instead of increase; and those given in this table are nearly the same as those printed by Wright, at London, in 1618, and by Ursinus, at Cologne, in 1624; though much more full, as will be seen by comparison. Thus, according to Wright, the log. sine of 0° 1′ is 8·142567, that of 0° 2′, 7·449421... while that of 89° 30′ is ·000038·1; and that of 89° 59′ only ·000000·1. Mr Leigh's manuscript, however, only commences with 5°; there is a gap from 32° to 33° 59′, and consequently also from 56° to 57° 59′ inclusive, and it ends with 85°.

The imperfect state of the tables, and the time at which the probable author lived, compared with the date at which the much more convenient tables to the base 10 were issued by Briggs and Vlacq (viz. 1624, 1628 for numbers, and 1633 for the Trigonometrical functions), lead us to suppose that if written out by Mr Leigh in his younger days, they were, at any rate, not thought by him worthy of completion; and it is not unlikely that he may have obtained them from some of his predecessors.

It may be well to insert here an example of the method in which Prof. De Morgan (Eng. Cyc. *Art.* Tables of Logarithms) shows that logarithms, such as are given in these tables, may be converted into natural, and also into modern logarithms. For example, we have the natural sine of 30° given as ·500000, but its log. is ·693147. Subtracting this from 6·907755, which is the *natural* logarithm of 1000, we have as remainder 6·214608. Dividing this by the modulus 2·30258 we get very nearly 2·69897, viz. the *common* logarithm of 500. Subtracting from this the *common* log. of 1000, viz. 3, we have 9·69897 as the *common* log. sin. of 30°.

*Vice versâ*, the natural sine of 5° is about ·087. Now the common log. of 87 is 1·9395193, and multiplied by the modulus, this becomes 4·465908, and this again subtracted from 6·907755 (the *natural* log. of 1000) leaves as remainder 2·441747. In the tables we find 2440058, a sufficiently near approximation, but it is to be observed that neither Wright nor this MS. insert any dot or comma after the first 2, though it will be seen by following the tables through that, on the system now adopted, it would be required.

took his M.A. degree at Oxford, and in 1664 the degree of S.T.B. at Cambridge. He went afterwards to Bishop Stortford, and much information respecting him will be found in Chauncy's *History of Herts.* The library which he formed for the benefit of the school is still in existence.

The *differentiæ* are, of course, the logarithms of the tangents; there is no table of natural tangents such as is given by Sherwin, but of course it could always be deduced from the table of their logarithms. In the manuscript itself, the whole table for the sines, differences, and cosines is given in one line, as in our present tables, but they could not have been all conveniently included in an octavo page.

Grad. 5°.

| | Sinus | Logarithmus | I | II | III | IIII | V | VI | VII | VIII | IX |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | X | XX | XXX | XL | L | LX | | | |
| 0 | 87156 | 2440058 | 553 | 1107 | 1660 | 2213 | 2767 | 3320 | 387 | 442 | 498 |
| 1 | 87445 | 2436738 | 551 | 1103 | 1654 | 2205 | 2757 | 3308 | 386 | 441 | 496 |
| 2 | 87735 | 2433430 | 549 | 1099 | 1648 | 2198 | 2747 | 3297 | 385 | 440 | 494 |
| 3 | 88025 | 2430133 | 548 | 1095 | 1643 | 2191 | 2738 | 3286 | 383 | 438 | 493 |
| 4 | 88315 | 2426847 | 546 | 1092 | 1638 | 2184 | 2730 | 3276 | 382 | 437 | 491 |
| 5 | 88604 | 2423571 | 544 | 1088 | 1632 | 2177 | 2721 | 3265 | 381 | 435 | 490 |
| | | Different. | | | | | | | | | |
| 0 | ........ | 2436245 | 557 | 1115 | 1672 | 2230 | 2787 | 3345 | 390 | 446 | 502 |
| 1 | ........ | 2432900 | 556 | 1111 | 1667 | 2223 | 2778 | 3334 | 389 | 444 | 500 |
| 2 | ........ | 2429566 | 554 | 1108 | 1661 | 2215 | 2769 | 3323 | 388 | 443 | 498 |
| 3 | ........ | 2426243 | 552 | 1104 | 1655 | 2207 | 2759 | 3311 | 386 | 441 | 497 |
| 4 | ........ | 2422932 | 550 | 1101 | 1651 | 2201 | 2752 | 3302 | 385 | 440 | 495 |
| 5 | ........ | 2419630 | 548 | 1097 | 1645 | 2193 | 2742 | 3290 | 384 | 439 | 493 |
| | Cosinus | | | | | | | | | | |
| 0 | 996195 | 3813 | 4 | 8 | 12 | 17 | 21 | 25 | 2·9 | 3·3 | 3·7 |
| 1 | 996169 | 3838 | 4 | 8 | 12 | 17 | 21 | 25 | 2·9 | 3·3 | 3·7 |
| 2 | 996144 | 3864 | 4 | 8 | 13 | 17 | 22 | 26 | 3·0 | 3·4 | 3·8 |
| 3 | 996118 | 3889 | 4 | 8 | 12 | 17 | 21 | 25 | 2·9 | 3·3 | 3·7 |
| 4 | 996093 | 3915 | 4 | 9 | 13 | 17 | 22 | 26 | 3·0 | 3·4 | 3·8 |
| 5 | 996067 | 3941 | 4 | 9 | 13 | 17 | 22 | 26 | 3·0 | 3·4 | 3·8 |

Grad. 30°.

| | Sinus | Logar. | I X | II XX | III XXX | IIII XL | V L | VI LX | VII | VIII | IX |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 500000 | 693147 | 84 | 168 | 252 | 336 | 420 | 504 | 59 | 67 | 76 |
| 1 | 500252 | 692643 | 84 | 168 | 251 | 335 | 419 | 503 | 59 | 67 | 75 |
| 2 | 500504 | 692140 | 84 | 168 | 251 | 335 | 419 | 503 | 59 | 67 | 75 |
| 3 | 500756 | 691637 | 84 | 168 | 251 | 335 | 419 | 503 | 59 | 67 | 75 |
| 4 | 501007 | 691134 | 84 | 167 | 251 | 335 | 418 | 502 | 58 | 67 | 75 |
| 5 | 501259 | 690632 | 84 | 167 | 251 | 335 | 418 | 502 | 58 | 67 | 75 |
| ... | ......... | ......... | ... | ... | ... | ... | ... | ... | ... | ... | ... |
| | | Different. | | | | | | | | | |
| 0 | ......... | 549306 | 112 | 224 | 336 | 448 | 560 | 672 | 78 | 90 | 101 |
| 1 | ......... | 548634 | 112 | 224 | 335 | 447 | 559 | 671 | 78 | 89 | 101 |
| 2 | ......... | 547963 | 112 | 224 | 335 | 447 | 559 | 671 | 78 | 89 | 101 |
| 3 | ......... | 547292 | 112 | 224 | 335 | 447 | 559 | 671 | 78 | 89 | 101 |
| 4 | ......... | 546621 | 112 | 224 | 335 | 447 | 559 | 671 | 78 | 89 | 101 |
| 5 | ......... | 545950 | 112 | 224 | 335 | 447 | 559 | 671 | 78 | 89 | 101 |
| ... | ......... | ... ... | ... | ... | ... | ... | ... | ... | ... | ... | ... |
| | Cosinus | | | | | | | | | | |
| 0 | 866025 | 143841 | 28 | 56 | 84 | 112 | 140 | 168 | 19·6 | 22·4 | 25·2 |
| 1 | 865880 | 144009 | 28 | 56 | 84 | 112 | 140 | 168 | 19·6 | 22·4 | 25·2 |
| 2 | 865734 | 144177 | 28 | 56 | 84 | 112 | 140 | 168 | 19·6 | 22·4 | 25·2 |
| 3 | 865589 | 144345 | 28 | 56 | 84 | 112 | 140 | 168 | 19·6 | 22·4 | 25·2 |
| 4 | 865443 | 144514 | 28 | 56 | 84 | 113 | 142 | 170 | 19·7 | 22·5 | 25·3 |
| 5 | 865297 | 144682 | 28 | 56 | 84 | 112 | 140 | 168 | 19·6 | 22·4 | 25·2 |
| ... | ......... | ......... | ... | ... | ... | ... | ... | ... | ... | ... | ... |

(3) "*On a Manuscript Volume of Astronomical Tables.*"

Dr Pearson then exhibited another manuscript volume, belonging to the same College, containing a number of Astronomical Tables, and apparently dating from the seventeenth century: as however it has been bound, since its completion, with a number of blank leaves at either end; and as there is no name or date whatever within the cover, we can only infer from the fact that it appears in the large catalogue of MSS. of 1698 under the same title, viz. *Astronomicæ Tabulæ,* as it now bears at the back, that it is not nearly so recent as that year. The suggestion at first offered itself, that as Horrox was a member of the College, and as it appears from Wallis's prefatory letter to his published works[1], that Jeremiah Shakerly, who afterwards obtained possession of his papers, and who published Astronomical Tables in 1653, followed up his studies on this subject, the volume might be one of the sources from which he inferred the approaching Transit of Venus of Nov. 24, 1639. It will be seen however on examination, that Shakerly's Tables could not well have been borrowed from this MS.

The Tables themselves are about XXXV. in number, though they are not separately numbered as we find them in the volume: they are very copious, each folio page containing about 60 lines of figures, with about 30 entries (reckoning *e.g.* $5^{\circ}.\ 17'.\ 26''$ as three entries) in each line: red ink is also used, as well as black, wherever necessary: the whole volume consists of 298 foll. or 596 pp., accurately ruled, and perfectly legible and distinct throughout. There is no index to the Tables; but their contents are always given at the top of the page.

Table I. gives the mean motions (*æquabiles motus*) of the precession of the equinoxes, and of the sun and their *anomalies,* for each century commencing with the year B.C. 4000. The position of the Sun at the equinox for that epoch, the whole Ecliptic being divided into Sextants of $60^{\circ}$, reckoned *eastwards* from the first point of Aries, is given as V. $9^{\circ}.\ 23'.\ 20''.\ 49'''.\ 9''''$, for the year 400 B.C. it is given as V. $59^{\circ}.\ 57'.\ 29''.\ 44'''.\ 7''''$, for the year 300 as —. $1^{\circ}.\ 21'.\ 13''.\ 18'''.\ 58''''$, which will assume the actual epoch when the Sun was in that point at the vernal equinox to have been 370 B.C. For the year 1800 the Sun's place at the Equinox is given as —. $30^{\circ}.\ 39'.\ 28''.\ 31'''.\ 2''''$; for the year 1900, —. $32^{\circ}.\ 3'.\ 12''.\ 5'''.\ 33''''$, from which *data* it can easily be seen how far the Table is in error.

Table II. gives the corresponding changes in the position of the Moon's ascending node for the same period.

Table III. gives similar computations for the Five greater Planets.

[1] Horrox, *Opera Post.* 1673.

Table IV. gives the mean motions already named for each year in a century.

Table V. gives the same motions

(*a*) for the period of 61 years, 1500 to 1560 A.D.
(*b*) for .......................... 1561 to 1621 ...
(*c*) for .......................... 1622 to 1682 ...
(*d*) for .......................... 1683 to 1743 ...
(*e*) for ........... of 57 years, 1744 to 1800 ...

Table VI. gives the same motions enumerated in the last Table for every day in the year.

Table VII. gives them, to the 12,960,000th part of a second of arc, marked thus ′′′′′, for hours and minutes of a day.

Table VIII. gives the Prosthaphæresis of the Equinoxes, and of the "eighth sphere."

Table IX. gives that of the Centre of the Sun.

Table X. gives that of the Sun's orbit.

Tables XI. XII. give the Prosthaphæresis for the Centre and the Orbit of the Moon.

Tables XIII. to XXII. give it for the Centres and Orbits of the Five Planets.

Table XXIII. seems designed for comparing the results of the last Tables.

Tables XXIV. to XXX. give the true daily motion of the Sun, Moon, and Planets.

Table XXXI. gives the Latitude of the Planets: the part referring to Mercury is much the longest, while that referring to Saturn only occupies thirty lines: the rest in proportion.

Table XXXII. gives the Latitude of the Moon throughout its Orbit.

Table XXXIII. shows how to compute the relative situations of the Planets.

Tables XXXIV., XXXV. show how to compute the situation of the Moon towards the Planets, and *vice versâ*.

The Tables are calculated to a considerable degree of minuteness; including frequently the 3600th part of a second of arc (marked thus —′′′′) as will be seen under Table I.: and in one case, viz. Table VII., I have noticed the 3600th part of this fraction; on the modern system these units would be expressed by 0″.000277 &c. and 0″.0000000771 respectively. An expert in the handwriting of the period might in this way approximately determine the date of the volume; but it is also possible that it might be inferred from the use of two expressions, which indicate the time of its compilation: they are the employment of the phrase "Caput Draconis" to mean the Moon's ascending node: and the occurrence of the expression "eighth sphere" (in Latin sometimes "*octava sphæra*" sometimes "octavus orbis") to indicate the

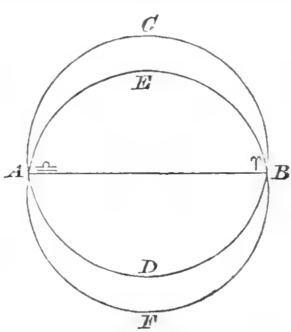
firmament or sphere of the fixed stars. These two expressions belong distinctly to the Ptolemean as opposed to the Copernican system of astronomy: and seem to prove that the Tables were composed at a time when the nomenclature at any rate, if not the principles of the former, had not been fully superseded by the latter.

The following passage is taken from Kircher's "Ars magna lucis et umbræ," p. 546. (Rome, 1646.)

Quare sive terra ipsae lunae opponenda sit, ut umbra sua, quae illud usque exporrigatur, eam obscuret; sive Luna Soli objiciatur, ut lumen ejus subtrahat terrae: necesse est utrinque Planetam sub ecliptica reperiri. Debes autem animo concipere in duobus eclipticae locis pari graduum intervallo transire in transversum lineam illam, sive circulum, per quem Luna fertur, qui ab ecliptica resiliens intervallum quoddam relinquit apertum; ubi vero conjungitur eclipticae una parte Caput Draconis, altera Cauda vocant Astronomi; melius Ptolemaeus, *συνδεσμὸν ἀναβιβάζοντα* et *καταβιβάζοντα*, id est nodum ascendentem et descendentem.

Similarly Gaspar Schott in his "Cursus Mathematicus," says, page 276 (Herbip. i.e. *Wurzburg*, 1661):

Duae illae intersectiones *A* et *B* vocantur a Graecis *σύνδεσμοι*, Nodi; à Latinis puncta ecliptica; eo quod luminaria in ipsis aut non procul ab ipsis conjuncta vel opposita, patiuntur eclipsin; ab Arabibus caput et cauda draconis dicuntur.........Duo puncta *C* et *B* quae maxime ab ecliptica recedunt et a nodis absunt 90 grad. appellantur limites quia sunt meta evagationis Lunae a solis directo tramite; ab Arabibus vero et vulgo dicuntur ventres draconis. Et punctum *C* est limes boreus, *D* austrinus. Arabis appellationis causa est quod segmenta illa *AECB* et *AF.DB* referunt similitudinem serpentum seu draconum, quorum caput et cauda sit in *A* et *B* ventres in *CE*, *DF*.




*AEBF* is the ecliptic. *ACBD* the moon's path.

Albategnius, the most celebrated of all Arab astronomers (IXth century), speaks only of *caput* and *cauda lunae* without the *draconis.*

Alfraganus, another Arab astronomer living about the same time, uses the expressions *caput* and *cauda draconis.*

The expression "eighth sphere" is all but, though not actually employed in the classical authors. It occurs first, as far as I am aware, in the *Timæus* of Plato, p. 36, in a passage of which the following is a translation, modelled on that by Mr Jowett: "This entire compound, *i.e.* the matter of the cosmical system, he divided lengthways into two parts, which he joined to one another at the centre like the letter $\chi\hat{\iota}$ and bent them into a circular form, connecting them with themselves and with each other at the point opposite to the point of contact: he then imparted to each a motion centred upon one and the same point, placing one of the circles in the interior, the other on the exterior. The exterior motion he styled the motion of the Nature of the Same: the interior motion, he styled that of the Nature of the Different. * * * To the Nature of the Same and the Like he gave dominion, for that he left it single and undivided: but the inner motion he split six times into seven unequal orbits, having their intervals in the ratios of two and three, i.e. three intervals of each; and bade their orbits move in opposite directions among themselves, three moving at a uniform velocity, and the remaining four at velocities differing among themselves and different to that of the three, but still not irregular."

At p. 38 of the same treatise, we have mention of the Morning Star, i.e. Venus, and Mercury, but the other planets have no names assigned to them.

The same views are expressed however by Cicero, and with more simplicity, *Tusc. Disp.* v. 24. "Quum totius mundi motus conversionesque perspexerit, *sidera*que viderit innumerabilia *cælo inhærentia* cum ejus ipsius motu congruere, certis infixa sedibus: *septem alia* suos quæque tenere cursus," and better still in the doubtful *Somnium Scipionis:* "Novem orbibus vel potius globis connexa sunt omnia: quorum unus est cælestis, extimus: * * * in quo infixi sunt illi qui volvuntur stellarum cursus sempiterni; cui subjecti sunt septem, qui versantur retro, contrario motu atque cœlum." The Sun, Moon, and five planets are then enumerated; the paragraph ending, "Nam ea quæ est media et nona, Tellus, neque movetur, et infima est, et in eam feruntur omnia suo nutu pondera."

It will be easily understood that a theory sanctioned by such high authorities as Plato and Cicero, retained its position until a very late period, and it will be found practically recognized by Goclenius, *Physica completa,* Lib. III. cap. II. ed. 1604, by Mo-

rinus, *Astrologia Gallica*, Lib. IV. cap. II. p. 96, and by Schott, *Cursus Mathem.* (Herb. 1661), Lib. VII. P. III. cap. VI. VII[1].

It will be remembered however that Schott and Kircher were writers unlikely to abandon the older terminology sooner than was necessary: on the other hand, towards the end of Kepler's *Epitome Astronomiæ Copernicanæ*, p. 908, ed. 1635, we read: "Quid fit de motibus his octavæ, nonæ, decimæque sphæræ: deque ipsis sphæris, in Astronomiâ Copernici? *Resp.* Dejicit illa supervacuas et vacuas stellis nonam et decimam: octavam, seu fixarum sphæram, mundo pro pariete extimo relinquit penitus immobilem: motus vero omnes tres, et quicquid insuper ex eo tempore novi deprehensum est, in unicum globum telluris confert." From a comparison of these authorities, especially from the way in which Kepler speaks of the expression we are discussing, and from their dates, we are hardly justified in supposing that the compiler of these Tables had entirely surrendered the older system, even if aware of its error in principle.

Before I conclude, I may add something on the word "Prosthaphæresis," the meaning of which seems to be given incorrectly in the ordinary Lexicons. The following is an extract from Ptolemy's *Synt. Math.* ed. Halma. Paris. 1813. Vol. I. pp. 272, 3. The word *anomaly* is of too common use in modern astronomy to need any explanation.

*Ὡς παλὶν τὴν τοῦ κανονίου διαγραφὴν ὁμοίαν γίνεσθαι τῇ ἐπὶ τοῦ ἡλίου στίχων μὲν μὲ (45), σελιδίων δὲ τριῶν, τῶν μὲν πρώτων δυὸ περιεχοντων τοὺς ἀριθμοὺς τῶν τῆς ἀνωμαλίας μοιρῶν, τοῦ δὲ τρίτου τὰς οἰκείως ἑκαστῷ τμήματι παρακειμένας προσθαφαιρέσεις, τῆς μὲν ἀφαιρέσεως γινομένης κατὰ τὴν ψηφοφοριαν ἐπὶ τὲ τοῦ μήκους καὶ τοῦ πλάτους, ὅταν ὁ τῆς ἀνωμαλίας ἀπὸ τοῦ ἀπογείου τοῦ ἐπικύκλου συναγόμενος ἀριθμὸς ἕως ρπ (180) μοιρῶν ᾖ, τῆς δὲ προσθέσεως, ὅταν τὰς ρπ μοίρας ὑπερπίπτῃ· καὶ ἔστι τὸ κανονίον τοῦτο.*

The passage is thus rendered by Halma.

Cette table, est, comme celle du soleil, de 45 lignes et en trois colonnes, dont les deux premières contiennent les nombres de l'anomalie, et la troisième les prostapheréses, ou nombres qu'il convient d'ajouter à chaque quantité ou d'en soustraire: d'en soustraire, tant de la longitude que de la latitude, si la somme des nombres de l'anomalie depuis l'apogée de l'épicycle, ne passe

[1] I am much indebted to Mr Arthur Schuster for the references to Morinus and Schott: and also for the extracts, &c., from Kircher and Albategnius: without his aid I might have lost much time in explaining the terms which they elucidate. I also owe Mr J. W. L. Glaisher thanks for assistance in the previous paper.

Κανονίον τῆς πρώτης καὶ ἁπλῆς ἀνωμαλίας τῆς σελήνης.

| Αριθμοι κοινοι. | | προσθαφαίρεσεις. | |
|---|---|---|---|
| Μοιραι (Degrees). | ΜΟΙΡΑΙ (Degrees). | ΜΟΙΡΑΙ (Degrees). | Ξ 60ths |
| ς (6) | τνδ (354) | ō (0) | κθ (29) |
| 1 β (12) | τμη (348) | ο (0) | νζ (57) |
| 1 η (18) | τμβ (342) | α (1) | κε (25) |

pas 180$^d$: d'y ajouter, si elle excède 180$^d$. Voici quelle est cette table.

So clear an explanation of the word needs no comment: it is merely an abbreviated compound of *Prosthesis* and *Aphæresis;* and corresponds to the modern alternative sign (±).

## *March* 11, 1878.

PROFESSOR LIVEING, PRESIDENT, IN THE CHAIR.

A communication was read from Mr R. MOON, commenting on a recent paper by Prof. Clerk-Maxwell, "On a paradox in the theory of attraction" (March 12, 1877).

A communication was read by

PROFESSOR CAYLEY, "*On a theorem of Abel's relating to a quintic equation.*"

The theorem in question is given, Œuvres t. II. p. 253, as an extract from a letter to Crelle dated 14th March, 1826, as follows:

"Si une équation du cinquième degré dont les coefficients sont *des nombres rationnels* est résoluble algébriquement, on peut donner aux racines la forme suivante:

$$x = c + Aa^{\frac{1}{5}}a_1^{\frac{2}{5}}a_2^{\frac{4}{5}}a_3^{\frac{3}{5}} + A_1a_1^{\frac{1}{5}}a_2^{\frac{2}{5}}a_3^{\frac{4}{5}}a^{\frac{3}{5}} + A_2a_2^{\frac{1}{5}}a_3^{\frac{2}{5}}a^{\frac{4}{5}}a_1^{\frac{3}{5}} + A_3a_3^{\frac{1}{5}}a^{\frac{2}{5}}a_1^{\frac{4}{5}}a_2^{\frac{3}{5}},$$

où

$$\begin{aligned}
a\ &= m + n\sqrt{(1+e^2)} + \sqrt{[h\,(1+e^2+\sqrt{(1+e^2)})]},\\
a_1 &= m - n\sqrt{(1+e^2)} + \sqrt{[h\,(1+e^2-\sqrt{(1+e^2)})]},\\
a_2 &= m + n\sqrt{(1+e^2)} - \sqrt{[h\,(1+e^2+\sqrt{(1+e^2)})]},\\
a_3 &= m - n\sqrt{(1+e^2)} - \sqrt{[h\,(1+e^2-\sqrt{(1+e^2)})]},
\end{aligned}$$

$$A\ = K + K'a + K''a_2 + K'''aa_2,\quad A_1 = K + K'a_1 + K''a_3 + K'''a_1a_3,$$

$$A_2 = K + K'a_2 + K''a\ + K'''aa_2,\quad A_3 = K + K'a_3 + K''a_1 + K'''a_1a_3.$$

Les quantités $c, h, e, m, n, K, K', K'', K'''$ sont des nombres *rationnels.* Mais de cette manière l'équation $x^5 + ax + b = 0$ n'est pas résoluble tant que $a$ et $b$ sont des quantités quelconques. J'ai trouvé de pareils théorèmes pour les équations du 7ème, 11ème, 13ème, etc. degré."

It is easy to see that $x$ is the root of a quintic equation, the coefficients of which are rational and integral functions of $a, a_1, a_2, a_3$: these coefficients are not symmetrical functions of $a, a_1, a_2, a_3$, but they are functions which remain unaltered by the cyclical change $a$ into $a_1$, $a_1$ into $a_2$, $a_2$ into $a_3$, $a_3$ into $a$. But the coefficients of the quintic equation must be rational functions of $c, h, e, m, n, K, K', K'', K'''$: hence regarding $a, a_1, a_2, a_3$, as the roots of a quartic equation, (the coefficients of this equation being rational functions of $m, n, e, h$) this equation must be such that every rational function of the roots, unchangeable by the aforesaid cyclical change of the roots, shall be rationally expressible in terms of these quantities $m, n, e, h$: or, what is the same thing, the group of the quartic equation (using the term "group of the equation" in the sense assigned to it by Galois) must be $aa_1a_2a_3$, $a_1a_2a_3a$, $a_2a_3aa_1$, $a_3aa_1a_2$. And conversely, the quartic equation being of this form, $x$ will be the root of a quintic equation, the coefficients whereof are rational and integral functions of $c, h, e, m, n, K, K', K'', K'''$.

To investigate the form of a quartic equation having the property just referred to, let it be proposed to find $\gamma, \gamma'$ functions of $e, h$, such that $\gamma^2 + \gamma'^2$ is a rational function of $e, h$, but that $\gamma^2 - \gamma'^2$, $\gamma\gamma'$ are rational multiples of the same quadric radical $\sqrt{\theta}$. Assume that we have

$$\gamma^2 - \gamma'^2 = 2p\sqrt{\theta},\ \gamma\gamma' = q\sqrt{\theta};\ \text{then } (\gamma^2+\gamma'^2)^2 = 4(p^2+q^2)\theta\,;$$

and in order that $\gamma^2+\gamma'^2$ may be rational, we must have $p^2+q^2=\lambda^2\theta$, or say $p^2+q^2=h^2\theta$; hence, $\theta=\frac{p^2}{h^2}+\frac{q^2}{h^2}$ must be a sum of two squares, or, assuming one of these equal to unity and the other of them equal to $e^2$, say $\theta=1+e^2$, we satisfy the required equation by taking $p=h$, $q=he$: viz. we thus have

$$\gamma^2-\gamma'^2=2h\sqrt{1+e^2},\ \gamma\gamma'=he\sqrt{1+e^2},\ \gamma^2+\gamma'^2=2h(1+e^2);$$

and thence also

$$\gamma^2=h(1+e^2+\sqrt{1+e^2}),\ \gamma'^2=h(1+e^2-\sqrt{1+e^2}),$$

the roots of these expressions, or values of $\gamma$, $\gamma'$, being such that

$$\gamma\gamma'=he\sqrt{1+e^2}.$$

Taking now $\alpha$ rational, $=m$ suppose, and $\beta$ a rational multiple of $\sqrt{1+e^2}$, $=h\sqrt{1+e^2}$ suppose, it is easy to see that the quartic equation which has for its roots

$$a,\ a_1,\ a_2,\ a_3=\alpha+\beta+\gamma,\ \alpha-\beta+\gamma',\ \alpha+\beta-\gamma,\ \alpha-\beta-\gamma',$$

has the property in question, viz. that every rational function of the roots unchangeable by the cyclical change $a$ into $a_1$, $a_1$ into $a_2$, $a_2$ into $a_3$, $a_3$ into $a$, is rationally expressible in terms of $e$, $h$, $m$, $n$.

It will be sufficient to give the proof in the case of a rational and integral function; such a function, unchangeable as aforesaid, is of the form

$$\phi(a,a_1,a_2,a_3)+\phi(a_1,a_2,a_3,a)+\phi(a_2,a_3,a,a_1)+\phi(a_3,a,a_1,a_2);$$

and if $\phi(a,a_1,a_2,a_3)$ contains a term $\alpha^m\beta^n\gamma^p\gamma'^q$, then the other three functions will contain respectively the terms $\alpha^m(-\beta)^n\gamma'^p(-\gamma)^q$, $\alpha^m\beta^n(-\gamma)^p(-\gamma')^q$, $\alpha^m(-\beta)^n(-\gamma')^p(\gamma)^q$; viz. the sum of the four terms is

$$=\alpha^m\beta^n[\{1+(-)^{p+q}1\}\gamma^p\gamma'^q+\{(-)^{n+p}1+(-)^{n+q}1\}\gamma^q\gamma'^p].$$

This obviously vanishes unless $p$ and $q$ are both even, or both odd; and the cases to be considered are 1°; $n$ even, $p$ and $q$ even, 2°; $n$ odd, $p$ and $q$ even, 3°; $n$ even, $p$ and $q$ odd, 4°; $n$ odd, $p$ and $q$ odd. Writing, for greater distinctness, $2n$ or $2n+1$ for $n$, according as $n$ is even or odd, and similarly for $p$ and $q$, the term is in the four cases respectively

$$
\begin{aligned}
&= 2\alpha^m\beta^{2n}\ \ (\gamma^{2p}\ \gamma'^{2q}\ \ + \gamma^{2q}\ \ \gamma'^{2p}),\\
&= 2\alpha^m\beta^{2n+1}\,(\gamma^{2p}\ \gamma'^{2q}\ \ - \gamma^{2q}\ \ \gamma'^{2p}),\\
&= 2\alpha^m\beta^{2n}\ \ (\gamma^{2p+1}\gamma'^{2q+1} - \gamma^{2q+1}\gamma'^{2p+1}),\\
&= 2\alpha^m\beta^{2n+1}\,(\gamma^{2p+1}\gamma'^{2q+1} + \gamma^{2q+1}\gamma'^{2p+1}).
\end{aligned}
$$

The second, third, and fourth expressions contain the factors $\beta(\gamma^2-\gamma'^2)$, $\gamma\gamma'(\gamma^2-\gamma'^2)$, $\beta\gamma\gamma'$ respectively; and the first expression as it stands, and the other three divested of these factors respectively are rational functions of $\alpha$, $\beta^2$, $\gamma^2$, $\gamma'^2$, that is, they are rational functions of $m$, $n$, $e$, $h$. But the omitted factors $\beta(\gamma^2-\gamma'^2)$, $\gamma\gamma'(\gamma^2-\gamma'^2)$, $\beta\gamma\gamma'$, $=2nh(1+e^2)$, $2h^2e(1+e^2)$, $nhe(1+e^2)$ are rational functions of $n$, $h$, $e$; hence each of the original four expressions is a rational function of $m$, $n$, $h$, $e$; and the entire function

$$\phi(a, a_1, a_2, a_3) + \phi(a_1, a_2, a_3, a) + \phi(a_2, a_3, a, a_1) + \phi(a_3, a, a_1, a_2)$$

is a rational function of $m$, $n$, $h$, $e$.

Replacing $\alpha$, $\beta$, $\gamma$, $\gamma'$ by their values, the roots of the quartic equation are

$$
\begin{aligned}
&m + n\sqrt{(1+e^2)} + \sqrt{[h(1+e^2+\sqrt{(1+e^2)})]},\\
&m - n\sqrt{(1+e^2)} + \sqrt{[h(1+e^2-\sqrt{(1+e^2)})]},\\
&m + n\sqrt{(1+e^2)} - \sqrt{[h(1+e^2+\sqrt{(1+e^2)})]},\\
&m - n\sqrt{(1+e^2)} - \sqrt{[h(1+e^2-\sqrt{(1+e^2)})]}.
\end{aligned}
$$

And I stop to remark that taking $m, n, e, h = -\frac{1}{4}, +\frac{1}{4}, 2, -\frac{1}{8}$ respectively, the roots are

$$
\begin{aligned}
&-\tfrac{1}{4} + \tfrac{1}{4}\sqrt{5} + \sqrt{[-\tfrac{1}{8}(5+\sqrt{5})]},\\
&-\tfrac{1}{4} - \tfrac{1}{4}\sqrt{5} + \sqrt{[-\tfrac{1}{8}(5-\sqrt{5})]},\\
&-\tfrac{1}{4} + \tfrac{1}{4}\sqrt{5} - \sqrt{[-\tfrac{1}{8}(5+\sqrt{5})]},\\
&-\tfrac{1}{4} - \tfrac{1}{4}\sqrt{5} - \sqrt{[-\tfrac{1}{8}(5-\sqrt{5})]},
\end{aligned}
$$

viz. these are the imaginary fifth roots of unity, or roots $r$, $r^2$, $r^4$, $r^3$ of the quartic equation $x^4+x^3+x^2+x+1=0$; which equation, as is well known, has the group $rr^2r^4r^3$, $r^2r^4r^3r$, $r^4r^3rr^2$, $r^3rr^2r^4$.

Reverting to Abel's expression for $x$, and writing this for a moment in the form

$$x = c + p + s + r + q,$$

the quintic equation in $x$ is

$$\begin{aligned}
0 &= (x-c)^5 \\
&+ (x-c)^3 \,.\, -5\,(pr+qs) \\
&+ (x-c)^2 \,.\, -5\,(p^2s+q^2p+r^2q+s^2r) \\
&+ (x-c)\;\;\,.\, -5\,(p^3q+q^3r+r^3s+s^3p)+5\,(p^2r^2+q^2s^2)-5pqrs \\
&+ (x-c)^0 \,.\, -\;\;\,(p^5+q^5+r^5+s^5) \\
&\qquad\qquad +5\,(p^3rs+q^3sp+r^3pq+s^3qr) \\
&\qquad\qquad -5\,(p^2q^2r+q^2r^2s+r^2s^2p+s^2p^2q),
\end{aligned}$$

and if we substitute herein for $p, q, r, s$ their values, then, altering the order of the terms, the final result is found to be

$$\begin{aligned}
0 &= (x-c)^5 \\
&+ (x-c)^3 \,.\, -5\,(AA_2+A_1A_3)\,aa_1a_2a_3 \\
&+ (x-c)^2 \,.\, -5\,(A^2A_1a_2a_3+A_1{}^2A_2a_3a+A_2{}^2A_3aa_1+A_3{}^2Aa_1a_2)\,aa_1a_2a_3 \\
&+ (x-c) \,.\, -5\,(A^3A_3a_1a_2{}^2a_3+A_1{}^3A_1a_2a_3{}^2a+A_2{}^3A_1a_3a^2a_1 \\
&\qquad\qquad\qquad\qquad +A_3{}^3A_2aa_1{}^2a_2)\,aa_1a_2a_3 \\
&\qquad\quad +5\,(A^2A_2{}^2+A_2{}^2A_3{}^2-A_1A_2A_3A_4)\,(aa_1a_2a_3)^2\,. \\
&+ (x-c)^0 \,.\, -\;\;(A^5a_1a_2{}^3a_3{}^2+A_1{}^5a_2a_3{}^3a_1{}^2+A_2{}^5a_3a^3a_1{}^2+A_3{}^5aa_1{}^3a_2{}^2)\,aa_1a_2a_3 \\
&\qquad\quad +5\,(A^3A_1A_2a_2a_3+A_1{}^3A_2A_3a_3a+A_2{}^3A_3Aaa_1 \\
&\qquad\qquad\qquad\qquad +A_3{}^3AA_1a_1a_2)\,(aa_1a_2a_3)^2 \\
&\qquad\quad -5\,(A^2A_3{}^2A_2a_1a_2+A_1{}^2A^2A_3a_2a_3+A_2{}^2A_1{}^2Aa_3a \\
&\qquad\qquad\qquad\qquad +A_3{}^2A_2{}^2A_1aa_1)\,(aa_1a_2a_3)^2,
\end{aligned}$$

viz. considering herein $A$, $A_1$, $A_2$, $A_3$ as standing for their values $K+K'a+K''a_2+K'''aa_2$, &c. respectively, each coefficient is a function of $a$, $a_1$, $a_2$, $a_3$ unaltered by the cyclical change of these values, and therefore a rational function of

$$m,\ n,\ e,\ h,\ K,\ K',\ K'',\ K'''.$$

*March* 25, 1878.

PROFESSOR LIVEING, PRESIDENT, IN THE CHAIR.

A communication was made to the Society by

Professors LIVEING and DEWAR, "*On the reversal of the lines of Metallic Vapours.*"

The apparatus employed by them is represented in the accompanying diagram. *D* is an iron tube half an inch wide and 2 feet long, the lower end closed, and coated with borax or fireclay. This was placed vertically in a small assaying furnace, *E* (shown in section) 9 inches square and about 12 deep with a good draft, and fed with Welsh coal so that a welding heat was attainable in it. A gentle current of hydrogen from the Kipp's apparatus *A*, purified and dried by passing through the tubes *B* and *C*, was led into *D* by a narrow brass tube reaching to the hot part of *D*. *F* is a small mirror by which the light from the interior of the hot tube was reflected on to the slit of the spectroscope *G*. In the earlier experiments the mirror was omitted, and the spectroscope held above the tube. Fragments of the metals examined were dropped into the tube *D*, and the consequent absorption of the light issuing from the hot bottom of the tube observed with the spectroscope. The most characteristic lines of thallium, indium and magnesium were thus seen directly reversed. The red line of lithium was only seen reversed when potassium, sodium and lithium chloride were all introduced into the tube together.

In the case of sodium besides the *D* lines they observed a dark line in the green with a wave length about 5510, and in the case of potassium a dark line with a wave length about 5730. These lines do not correspond with any known emission lines, though they are in each case near to, but more refrangible than, well-known emission lines of those elements. The channelled spectra described by Roscoe and Schuster (*Proc. R. S.* v. 22) were also seen sometimes, but the above-mentioned lines were seen at times when the channelled spectra were not visible. Besides observing potassium and sodium in the iron tubes, they examined the absorption produced by those metals volatilized in glass tubes about 3 inches long, of the form *H* in the accompanying diagram, looking through the length of the tube at a lime light. The ends of these tubes were drawn out so as to present approximately plane faces at the ends, one end being drawn out into a narrow tube by which

the air could be exhausted and other gases introduced at will. In this form of tube the metals could be kept in the state of vapour in the bulb for some time without obscuring the ends. The same absorption lines were seen in the glass tubes as had been observed in the iron tubes. The authors remarked that the density of the vapour of sodium at the temperature of the iron tubes appeared abnormally great, judging from the quantity of the metal which was needed to fill the hot part of the tube with the vapour. As soon as the vapour rose to the level of the end of the small tube conveying the hydrogen, a black cloud of sodium vapour condensed by the cool hydrogen completely obscured all the light from the lower part of the tube, and a smoke of soda issued with the hydrogen from the open end of the tube.

They did not succeed in getting any absorption spectra produced by zinc, cadmium, lead or silver, though all these, except perhaps silver which gave no distinctly recognisable vapour, were volatilized in the tubes.

## *May* 6, 1878.

PROFESSOR CAYLEY, VICE-PRESIDENT, IN THE CHAIR.

A communication was read by Prof. CLERK-MAXWELL, "*On Boltzmann's Theorem on the average distribution of energy in a system of material points.*"

The publication of this paper is deferred for the present.

## *May* 20, 1878.

PROFESSOR LIVEING, PRESIDENT, IN THE CHAIR.

Rev. E. HILL, "*An elementary discussion of some points connected with the Influence of Geological changes on the Earth's Axis of rotation.*"

A paper has lately been read before the Royal Society by Mr G. H. Darwin, in which he investigates the effect which will be

produced on the position of the Earth's axis by small deformations in its shape. My object in these notes is to give comparatively elementary discussions of some propositions which he has there demonstrated.

I. *The Axis of Rotation is fixed in space.*

The angular momentum about an axis fixed in space can only be changed by external force. From Newton's 3rd Law, or by the Conservation of Angular Momenta, no internal actions can produce any resultant momentum in a body. To change the axis of rotation a new momentum must be produced to be compounded with that previously existing. But no internal actions can produce this. Thus no deformation produced by internal action can of itself change the direction of the axis in space.

And there is no external force to produce any considerable alteration. The attractions of the sun or moon cannot do it. In treatises on Precession and Nutation where the problem of their effect on a rotating spheroid is worked out, it is shown that there is no secular change in the obliquity. Hence any deformation which leaves the earth a spheroid rotating about its axis of figure, can only alter the magnitudes of periodic changes, and cannot produce any secular alteration of obliquity. We will show hereafter that the deformations we deal with cannot make it rotate about an axis different from that of figure.

If the figure cease to be a spheroid, this appeal to the Precessional Theory must be modified. We may argue as follows:

On a sphere's rotation the sun's attraction can produce no effect, for it can exert no couple. On a spheroid rotating about its axis of figure, the sun's attraction does exert a couple at right angles to the plane of rotation, but none in this plane. The rate of rotation therefore remains constant, and we know that the above couple tending to decrease the obliquity, cannot permanently alter it. In an Ellipsoid this couple will vary periodically in magnitude, but this can only introduce a new periodic term into the obliquity. A couple will however arise about the axis of rotation, which may produce a permanent effect on the rate, and, if so. possibly a secondary effect on the obliquity. Since however the deviation from a spheroid is by supposition minute, the change of rate of rotation must be very minute, and much more so the above secondary effect on the obliquity, if indeed it exist at all.

This discussion is not very satisfactory, and it will be better to use Mr Darwin's analysis. He also takes the spheroidal motion as the first approximation, and his results also show that the secular change in the obliquity only exists during the process of deformation.

II. *The axis of figure must continue sensibly coincident with the axis of rotation.*

Suppose the earth's shape to have so changed, that the axis of rotation no-longer coincides with that of figure. The instantaneous axis of rotation, though as we have shown fixed in space, will no longer be stationary in the body. It will be so moving that the momental ellipsoid rolls on the tangent plane at its instantaneous extremity. The path traced out by the instantaneous axis on the surface of the momental ellipsoid (the polhode) will be an oval with the end of the axis of figure in its centre. The momental ellipsoid was originally a spheroid, and is therefore if slightly deformed approximately or even actually still such. Hence the oval will be approximately or actually a circle. Thus the instantaneous axis may be considered to trace out on the momental ellipsoid, and therefore also on the earth itself, a circle round the pole of figure. We may notice that in space it is the axis of figure which is moving: the axis of rotation is fixed, as we showed in Section I.

Now if the pole of figure, owing to Geological changes, is uniformly shifting its position in the body along a straight line, and if the pole of rotation is at any instant thus revolving uniformly in a circle about it, the path (the roulette) it traces out in the surface must be a trochoidal curve. The particular shape is decided by the consideration that initially these two poles were coincident. This shows that the trochoid is a cycloid. The path then will be a series of cycloids, whose bases lie in the line along which the axis of figure is being shifted.

With the present shape of the earth, and the present value of its moments of inertia, the period in which the pole of rotation would describe its circle round the pole of figure would be about 300 days. Small deformations of the earth can only produce small changes in this period. Thus each cycloid would be completed in about 300 days, and the poles of figure and rotation would coincide at intervals of the same length. Their greatest separation would be at the middle of these intervals, and would bear the same ratio to the distance moved over in that time by the pole of figure, which the diameter of a cycloid does to its base, i.e. $1 : \pi$. Internal changes such as we are at present acquainted with, able to elevate or depress parts of the surface but a few feet in a century, cannot in 300 days shift the pole of figure over a serious distance. The widest separation between this pole and that of rotation is we have shown less than one-third of this distance. Thus the two poles, however they wander over the body, must remain sensibly coincident. Evidently also our preliminary assumption that the pole of figure should be moving in a straight line becomes wholly unnecessary.

III. Mr Darwin suggests that although this possible separation must be so minute, it must yet introduce stresses into the rotating earth, which might be relieved by earthquakes restoring coincidence. This seems probable. In this way an 150 day or 300 day period in earthquakes might be produced. It would be interesting to investigate whether such a period may not now exist.

Mr Darwin calculates the path of the pole on the surface supposing the earth plastic. The general problem can hardly be solved without analysis, but the following cases may be noticed.

The stress will be proportional to the separation. Suppose that whenever the stress reaches a certain amount, the crust yields suddenly, so as to restore the coincidence of the poles. The pole of figure will thus move a uniform distance along its path, and then be deflected a uniform distance at right angles to its previous track. If we suppose these distances very small, we may approximately represent the motion of the pole, by supposing it to possess, in addition to its original velocity, another at right angles to this, and also uniform. If we suppose that the Geologic changes were shifting the pole towards a certain point, its path, as resulting, from the causes above mentioned, would be an equiangular spiral about that point. Since the pole of rotation, starting on its cycloid, moves at right angles to the motion of the pole of figure, if the crust yields at this instant, the pole of figure follows it. This is the case of plasticity amounting to fluidity, and makes the spiral become a circle.

Should the earthquake not restore complete coincidence, the pole would start on an epitrochoid instead of a cycloid, but if the earthquakes took place as before at a definite degree of stress, and diminished the separation by a constant fraction, the path would be as before an equiangular spiral. This gives a rough idea of the effect of viscosity, and seems to indicate that the path of the pole would on that supposition be some kind of spiral.

IV. Expansion and contraction will be less effective in changing the position of the principal axes than transference of surface matter.

The parts of the moments and products of inertia due to any particle will be of two dimensions in the co-ordinates of its position. In consequence, they must vary as the square of its distance from the origin. To elevate the surface at any point by expansion, is equivalent to removing matter from the interior and depositing it on the surface. We may suppose this removal of matter to take place along radii. Such a removal will increase the moments or products of inertia of the body; but not to the same extent as the addition of an equal quantity to the surface: for there is a loss by subtraction, as well as a gain by addition. Thus

the change in these quantities produced by the removal of a particle to the surface, from an original depth of 40 miles, would be less than the change due to the simple addition of such a particle to the same point, in the ratio of $4000^2 - 3560^2 : 4000^2$, or of $1 - (1 - \frac{1}{100})^2 : 1$, or of $\frac{2}{100} : 1$ approximately, that is $1 : 50$.

Hence Mr Darwin's conclusion clearly follows, that expansion and contraction are vastly less effective in changing the position of the axes than transference of surface matter, at all events when they occur within any moderate depth. We cannot assert *a priori* that the changes of angle are in simple proportion to the changes of moments and products of inertia. Mr Darwin's analysis shows that they are, and the above result agrees roughly with one of his calculations.

## Election of Honorary Members.

The following have been recently elected Honorary Members of the Society:

*On the Home List.*

Dr T. A. Hirst, F.R.S., Royal Naval College, Greenwich.
C. W. Siemens, Esq., D.C.L., F.R.S.

*On the Foreign List.*

| | As distinguished in |
|---|---|
| Prof. F. Klein, Munich, <br> Prof. A. Mannheim, Paris, | *Mathematics.* |
| Prof. S. Newcomb, Washington, | *Astronomy.* |
| Prof. A. Kekulé, Bonn, <br> Prof. H. Sainte-Claire Deville, Paris, | *Chemistry.* |
| Prof. R. J. E. Clausius, Bonn, <br> Prof. A. Cornu, Paris, | *Physics.* |
| Prof. Kowalevsky, Odessa, <br> Prof. C. Ludwig, Leipsic, | *Biology.* |

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The following have been elected Fellows of the Society:

| | |
|---|---|
| Feb. 11, 1878. | T. W. Bridge, B.A., Trinity College. |
| „ 25, „ | D. McAlister, B.A., Christ's College. |
| Mar. 11, „ | H. J. H. Fenton, B.A., Christ's College. |
| „ „ „ | W. N. Shaw, B.A., Emmanuel College. |
| „ 25, „ | E. W. Hobson, B.A., Christ's College. |
| May 20, „ | Adam Sedgwick, B.A., Trinity College. |

The following have been elected Associates:

| | |
|---|---|
| Mar. 25, 1878. | H. Middleton, Unattached Student. |
| „ „ „ | W. H. Waters, Christ's College. |

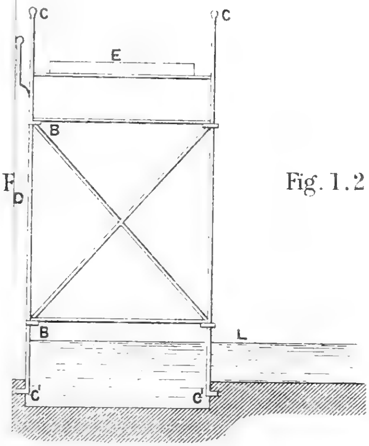
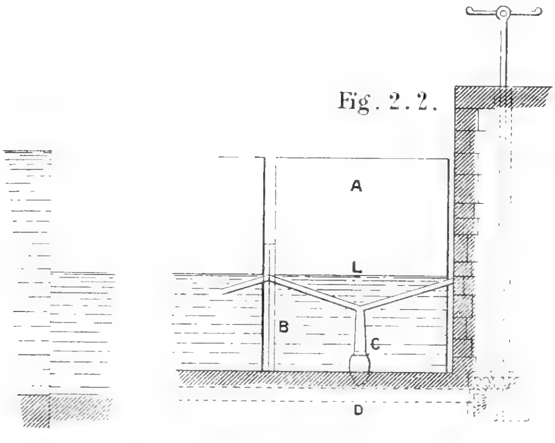



Fig. 1.2.

Fig. 2.2.



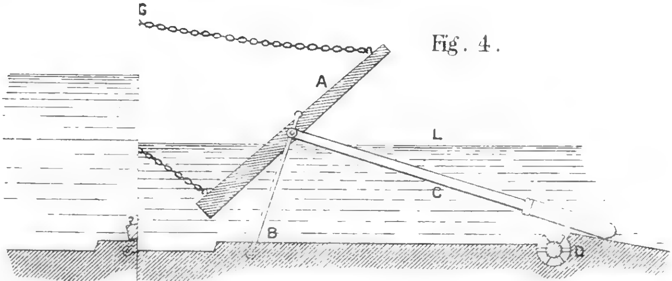




Fig. 4.

G.C.C. DEL^T.

Fig. 1.

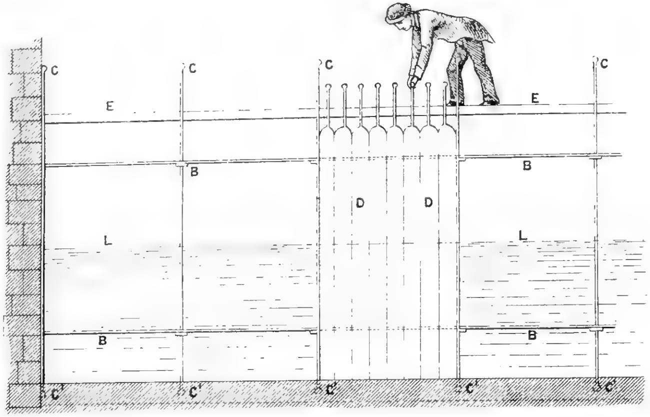



Fig. 1.2.

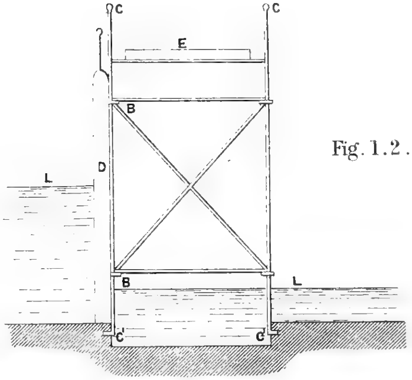



Fig. 2.

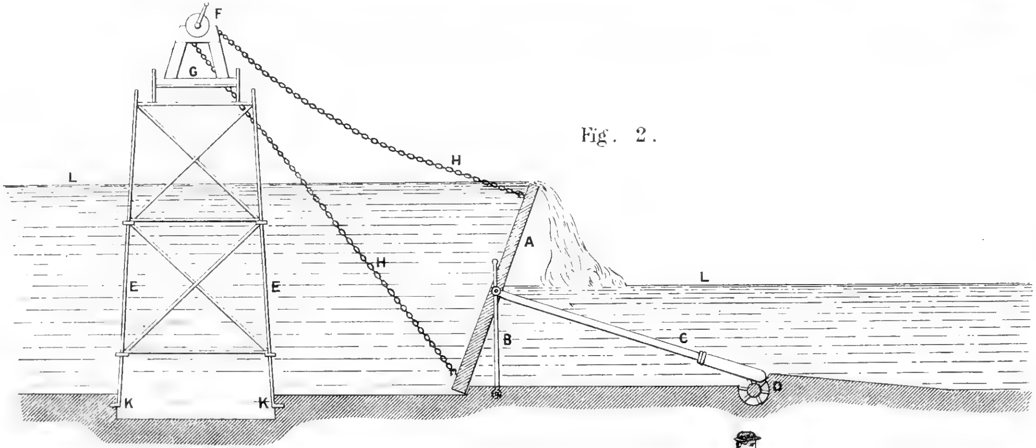



Fig. 2.2.

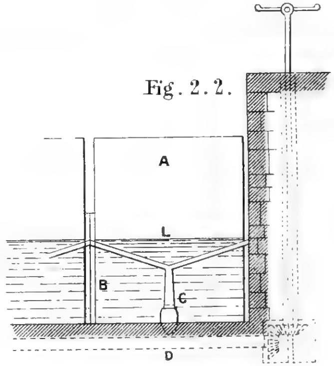



Fig. 3.

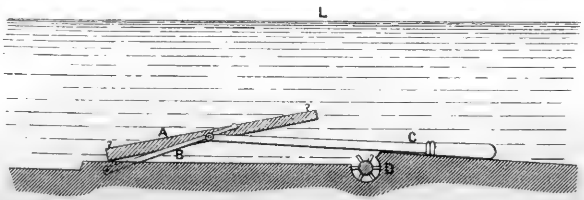
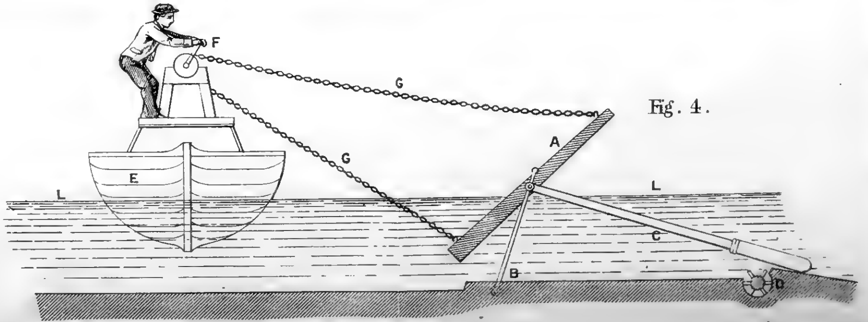
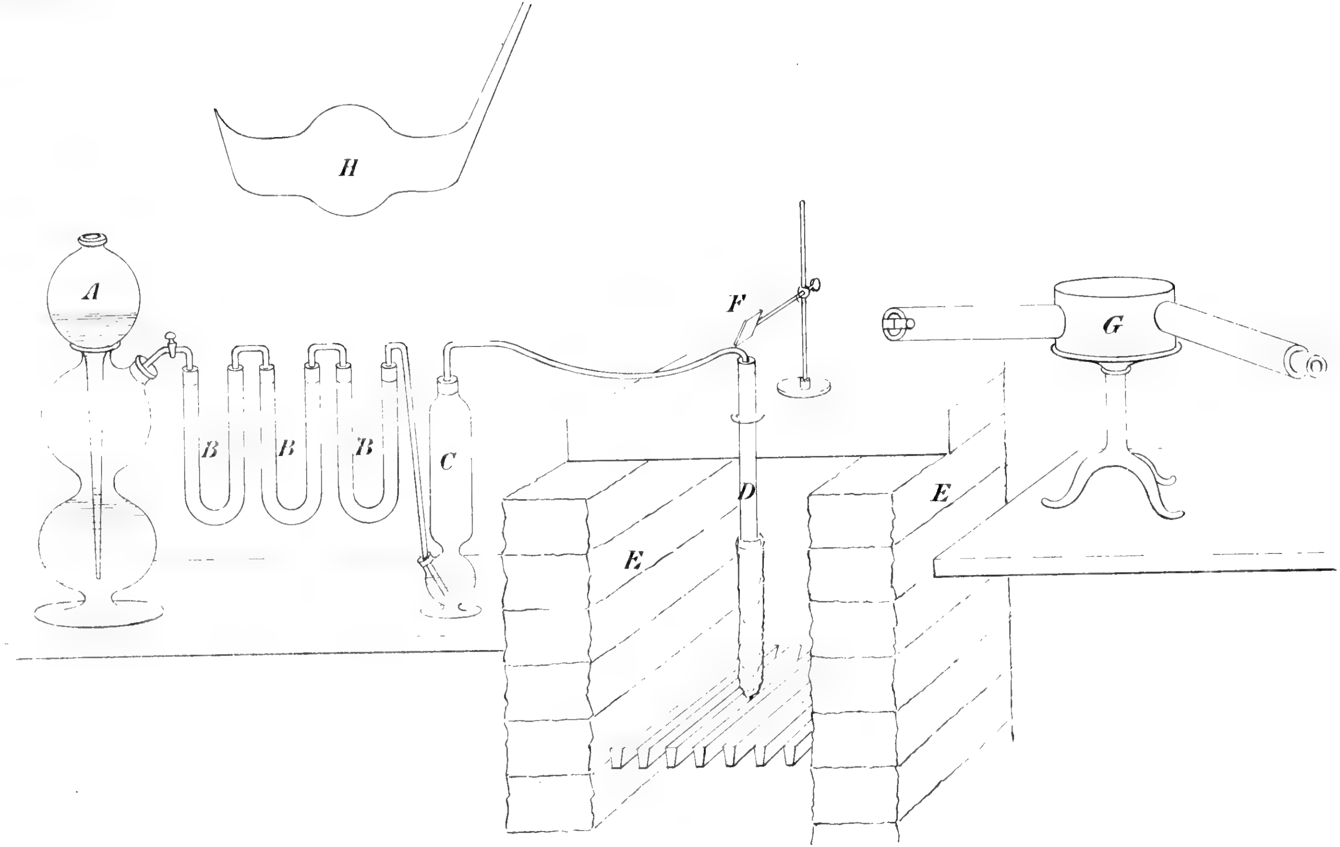



Fig. 4.

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# PROCEEDINGS

OF THE

# Cambridge Philosophical Society.

*October* 21, 1878.

PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

Mr S. H. Vines, B.A., Christ's College, Mr F. C. Lambert, B.A., Downing College, and Mr F. B. de M. Gibbons, B.A., Gonville and Caius College, were balloted for and duly elected Fellows of the Society.

The following communication was made to the Society:—

DR PEARSON, *On a series of lunar distances.*

THE object of this paper is to discuss a series of Lunar distances taken during the years 1875-7, with the view of inviting fresh attention to an old and hitherto not quite satisfactory problem.

The entire series consists of 250 distances: of these only 200 are considered in this paper. They were taken for the most part in the grounds of Emmanuel College, and a small number at Abington, a short distance from Cambridge.

By referring to Vol. II. of the Society's *Proceedings*, pp. 414—18, a full description will be found of the instrument, and of the kind of errors which suggested farther investigation: it need only be mentioned here that the first place of observation is situate as nearly as possible in $52^0\ 12'\ 10''$ N., $0^h\ 0^m\ 29^s$ E.; the second in $52^0\ 7'$ N. and $0^h\ 0^m\ 56^s$ E. by the one-inch map. All the observations taken in the first spot, and a few of those taken in the second, were immediately referred to a Chronometer, rated partly by the Post Office Telegraph, partly by the Society's clock as set by the time of the Observatory: and though these two *data* do not exactly coincide, the discrepancy on this account cannot have amounted to more than 4 or 5 seconds. Of the observations taken at Abington, most depend on a fairly going watch, corrected by

the Local Time obtained from the Sun, or some other luminary: the results obtained from Lunars taken on time thus established, as compared with those taken on time certainly known, seem to prove that no serious error can be fairly suspected on this account.

It is stated in the paper referred to above that no very long series of observations had led to a conviction that in certain cases the error, if any, in the observed distance was almost always one of defect, and in certain others one of excess; and that the determining cause of error was to be found in the relative positions of the Sun or Star, and of the Moon towards the Meridian. The problem, as far as is known to the author of this paper, has always been made simply to depend on the relative altitudes of the two luminaries: but the methods of solution hitherto used do not seem to have given really satisfactory results. This may be assumed on various grounds: in the first place, this method of ascertaining longitude at sea is now actually very much disused: it is not given in the *Berliner Jahrbuch*, the recognized German nautical Almanac: works on astronomy admit the difficulty of applying it in practice: Captain Toynbee, F.R.A.S., in the *Nautical Magazine* for Feb. 1850 (a paper of which there is an abstract in the *Monthly Notices* of the R.A.S.), distinctly states that he had found by experience that Lunars taken E. of the Moon always give a result thirty or forty seconds different from those taken W., though his mean result, he says, was always satisfactory; and until the early part of this century all East Indian Longitudes were in error about $7' \, 0''$ or $28^s$ to the East; a result which very fairly agrees with the errors resulting from this series of observations. A method of determining longitude, at once so useful and so scientific, so simple and at the same time so *nearly* exact, ought not to be abandoned because it has not as yet been brought to perfection.

We will now proceed to give an outline of the results actually obtained.

Each one of the 200 distances now under discussion is either a mean of three or two, or else depends on only one observation, though there is no reason to think that the last are in any serious degree less trustworthy than the others. The observed distance and the Greenwich time being thus obtained for each example, the altitudes have in all but a very few cases been computed, *not* observed: and the distance then *cleared* by Borda's formula. An example is here given of the method employed, in order to satisfy those who may have given attention to the different ways in which the problem itself has been solved. It should however be noted that the following observation, and indeed all those for the year 1876, have been worked out by the aid of the *Connaisance des Tems,* and to the meridian of Paris: it was thought that by working for one year on French for another on English authority, any

exception as to the inaccuracy of the Nautical Almanac might be avoided; it seems clear, however, that both volumes are immediately based on the same Tables, and perhaps the idea of a discrepancy will seem superfluous to any one acquainted with the subject.

July (13) 14, 1876.

| | | | |
|---|---|---|---|
| P.M.T. | 7 44 30 a.m. | | |
| L.M.T. | 7 35 38 ... | | |
| Eqn. T. | 5 34 | | |
| Co. H.A. | 7 30 4 | s. sq. | 9·839777 |
| ☉ P.D. | 68 21 40 | sin | 9·968261 |
| Co. lat. | 37 47 50 | sin | 9·787368 |
| | 106 9 30 | | |
| | 77 45 2 | s. sq. | 9·595406 |
| | 183 54 32 | | |
| | 28 24 28 | | |
| | 91 57 16 | sin | 9·9997473 |
| | 14 12 14 | sin | 9·3898271 |
| ☉ Z.D. | 59° 22′ 0″ | s. sq. | 9·3895744 |
| ☉ True Alt. | 30° 38′ 0″ | | |
| | 1 28 | | |
| ☉ App. do. | 30° 39′ 28″ | | |

| | |
|---|---|
| ☉ and ☾ (3 obss.) | 93° 10′ 13″ |
| ind. error | 20 |
| ☉ s. d. | 15 46 |
| ☾ s. d. | 15 48 |
| | 93 42 7 |

☾ h.p. 57′ 9″

B. 30.5
Th. 72° F.

| | | |
|---|---|---|
| S.T. | 7 30 3 | |
| L.M.T. | 19 35 38 | |
| | 3(27) 5 41 | |
| ☾ R.A. | 1 6 6 | |
| ☾ H.A. | 1 59 35 | |
| | 10 0 25 | 9·970099 |
| ☾ P.D. | 80 26 16 | 9·993923 |
| Co. lat. | 37 47 50 | 9·787368 |
| | 118 14 6 | |
| | 97 22 14 | 9·751390 |
| | 215 36 20 | |
| | 20 51 52 | |
| | 107 48 10 | 9·9786892 |
| | 10 25 56 | 9·2578519 |
| ☾ Z.D. | 49° 3′ 56″ | 9·2365411 |
| ☾ True Alt. | 40° 56′ 4″ | |
| | −(42′ 4″+26″+3″) | |
| ☾ App. Alt. | 40° 13′ 31″ | |

(To reduce or *clear* the observed distance.)

| | | | |
|---|---|---|---|
| Dist. | 93° 42′ 7″ | | |
| ☉ a. a. | 30 39 28 | sec. | 10·0653863 |
| ☾ a. a. | 40 13 31 | sec. | 10·1171846 |
| | 164 35 6 | | |
| | 82 17 33 | cos | 9·1274803 |
| | 11 24 34 | cos | 9·9913318 |
| ☉ t. a. | 30 38 0 | cos | 9·9347235 |
| ☾ t. a. | 40 56 4 | cos | 9·8782113 |
| | 71 34 4 | | 2)9·1143178 |
| | 35 47 2 | | |
| | 68 51 22 | cos | 9·5571589 |
| | 104 38 24 | sin | 9·9856658 |
| | 33 4 20 | sin | 9·7369506 |
| | | | 2)9·7226164 |
| | 46° 36′ 14″ | sin | 9·8613082 |
| | 93° 12′ 28″ | | |
| Dist. at 9 a.m. | 92 35 50 | Diff. for p. l. (+) 5s. | |
| | 36′ 38″ | p. l. 6914 | |
| | | 2937 | |
| | 1h 12m 2s | p. l. 3977 | |
| | 7 47 58 | | |
| | 5 | | |
| P. M. T. | 7 48 3 | a. m. by Obs. | |
| ,, | 7 44 30 | ,, by Chron. | |
| error | 3 33s (=1′ 48″ arc). | | |

Observed distance too *small*, because the Moon is approaching the Sun.

It is of course usual in working out a result from a Lunar, to find by it the Longitude in time of the place of observation: in this paper, on the contrary, the task has been, as the Longitude is known, to deduce the error of observation: this is strictly speaking the error in *arc*, the error in longitude depending on this, if it is a real error in the instrument, and probably, in any case.

It is impossible, in a brief article like the present, to give the elements of each separate observation included in the series, though it is hoped that they may be in the press before long. A general analysis however of the results can easily be given: classing them in groups of about 40 each, and considering the first of these only at present, it was found that there were 29 cases in which the measured distance was in defect of the theoretical distance, and 12 in which it was in excess. Assuming the rule given in p. 417 of the paper referred to to be correct, this result agrees exactly with what might be expected: it being almost always most convenient, especially for a beginner, to take Lunars, especially from the Sun, under such circumstances as will give this result: while the example of India, founded on observations made at Madras, seems to imply this probable facility, supposing that they were mostly taken from the Sun on the new Moon, these being as easily and certainly taken as any others in the northern hemisphere[1]. In the four remaining groups, the proportions are 24 to 17, 28 to 15, 25 to 17, 17 to 14: giving a total of 123 observations in defect, and 75 in excess. Rejecting 3 or 4 certainly questionable results, the greatest errors are 2′ 59″ in defect, and 2′ 48″ in excess. In the first group the mean error in defect is 1′ 18″, the mean error in excess is 0′ 58″. In the four remaining groups the mean errors in defect are 1′ 24″, 1′ 0″, 0′ 35″, 0′ 50″; while the mean errors in excess are 0′ 45″, 0′ 59″, 0′ 46″, 0′ 51″[2].

The mean error we obtain from the five groups taken together is 1′ 1″ in defect, and 0′ 52″ in excess. The longitude of a place established from these observations will therefore be in error to the amount which will be indicated by the difference of these final errors, or about $4\frac{1}{2}''$. This error in the measured distance implies an error of about $9^s$ or 2′ 15″ geographical longitude. As noticed above, the error at Madras previous to the precision of recent times, amounted to about three times as much, or $28^s$. The first group however would have given an error twice as great, or about 4′ 30″ of geographical longitude in the same direction as that

[1] It is stated in Markham's *Indian Surveys*, that a series of about 800 (? sets of) lunar distances taken from 1787 onwards had made the longitude of the Observatory at Madras to be reputed in 1830 at 80° 21′ 25″ E. It is now fixed at 80° 14′ 20″, giving an error in former years of 7′ 5″.

[2] If two unusual errors of 3′ 12″ and 3′ 47″ be omitted, the means in excess for the third and fifth groups will be 50″ and 37″ instead of 59″ and 51″.

deduced in India, a fair evidence that Lunars taken in the simplest and easiest way will generally give this result north of the Line.

It has been already remarked that by taking Lunars E. and W. of the Moon, a writer on this subject has stated that the mean of the sets had given him a satisfactory result; and he adds with truth, that in this way the effects of a *constant* error in his sextant would be eliminated: errors in observation taken E. and W. affecting Greenwich Time in opposite ways. I venture to think that the meridian only is the true point of reference, as I stated in my previous paper; and that, if both the luminaries are on the same side of it, the error will generally be in defect or excess, according as the Moon is nearer to or more distant from it: when they are on different sides, I am unable as yet to give any decided view, but it seems to me that the same principle generally rules. Hitherto the problem has been always treated as depending only on the altitudes of the two bodies: and that this is very fairly exact cannot be questioned; but it is not entirely so, as far as I can see; and I believe that by considering their positions with reference to the meridian, we obtain a new point of departure which may perhaps lead to a satisfactory conclusion.

Now of the observations in question, in about 92 out of 200 cases, both the luminaries were on the same side of the meridian; in 38 towards the East, in 54 towards the West: while of those in which they were on opposite sides, in 58 the Moon was to the West, and in 48 to the East: and by giving the errors in defect or excess in each of these categories, we shall have an approximately accurate means of deciding how far the rule proposed above may be considered true. Let us then take first those cases in which the Sun or star, and also the Moon, were both to the West of the meridian, as it was mainly from these cases that the rule suggested itself. Of these there are 28 instances in which the Moon was nearest to the meridian: and in 24 of these the measured distance is in defect of what it ought theoretically to be, while in three out out of the four exceptions her altitude was under $10^0$ and less than that of the Sun. On the other hand, of 23 cases in which she was the more remote of the two bodies, in 21 cases the error is in excess, and the two exceptions can cause no surprise, even supposing them to be errors of observation. We will take next the examples in which the Moon was to the East of the meridian, the Sun or star being on the opposite side. Out of 43 cases in which the hour angle of the Moon was less than that of the other luminary, I find only seven in which the measured distance was in excess: and in 4 of these the distance was very great, viz. $128\frac{1}{2}^0$ and $129^0$, while in two others it was about $116^0$, nor did the error in any case amount to more than $31''$. As there are only four examples in which the Sun or star was nearest to the meridian,

but little can be inferred from the fact that two cases make for and two against the rule holding for distances across the meridian. Thirdly, when the Moon has been to the West, and the Sun or star to the East, there are 25 cases when the error has been in defect, the Moon being nearest to the meridian; and nine in which it has been in excess; on the other hand, the Moon being the more remote of the two luminaries, the error is (correctly) in excess in only eleven against ten cases: but these ten exceptions are nearly all taken from the Sun, and demand a special allowance which will be noticed farther on. It will thus result that the rule which seemed almost universally to hold when both the heavenly bodies were on the West of the meridian, frequently applies when they are on opposite sides, though with less certainty. We now come lastly to the observations in which both the luminaries were to the East of the meridian. And of these it must be premised that they are much the least convenient to take in practice. When the Moon is near its rising and more distant from the meridian, the stars nearer to the meridian will seldom reflect distinctly; while the construction of any instrument like a sextant, requires the object viewed directly to be the one to the left hand; and again to work on the Moon in its last quarter, when its light is become tolerably weak, involves using the 'small' hours of the morning; nor have the observations I have taken on the Sun viewed directly been very satisfactory, as the construction of my circle prevents the reflected Moon being very distinct. For these reasons the E. observations are the least conformable of all to any rule, but the special analysis of them which is subjoined will shew that the discrepancies are not wholly unaccountable. Of 25 cases in which the Moon was nearer to the meridian, 14, with an average error of 48″, gave the measured distance too small, and 12, with an average error of 42″, gave the distance too large; most of these last, however, were taken on Arcturus, α Arietis, and Pollux, which have respectively a N. Dec. of 20°, 22°, 29° nearly, an important element in the reduction of the observations. On the other hand, of 10 cases in which the Moon was farthest from the meridian, 7 gave a mean error of 37″ agreeable to the rule, and 3 a mean error of 32″ in a contrary sense. If the inconvenience of taking these Eastern observations referred to above be taken into account, the result cannot be fairly said to negative the conclusions drawn from those taken in the West.

It is not pretended that the observations themselves are rigidly exact; most of the distances taken on the Sun ought to be augmented by a small amount, perhaps 10″—15″, on account of the fact that the diameter of the Sun as measured by a small instrument is generally too great: this error seems to be due to the breaking up of the rays in the telescope. I have also entirely neglected

two small corrections, that for the contraction of the Moon's semi-diameter, because in most cases it will not amount to 10″, and would generally operate to increase the error; that for the spheroidal figure of the earth, because I feel no security that the methods usually given for correcting this are to be relied on; at any rate, a method formerly given in the *Nautical Almanac*, 1829-32, has been since omitted, and on reducing the examples given there by Chauvenet's method, I obtain quite different results. But it should be clearly understood that no mode of reducing the distance which will give a really different result to that employed in computing these observations has yet been published; it is not stated on what principle the system adopted in the folio volume of 1772 was based[1]; but by working out the examples given there according to the formula adopted in the example which I have printed, it will be found that the results are as nearly as could be expected identical. Nor does it seem that better success can be expected from the method given by Bessel in the *Astronomische Nachrichten* of 1832.

Bessel's plan is very elaborate; assuming a Greenwich date as nearly exact as possible, the true apparent distance must then be computed, the result compared with the observed distance, and the error in the *assumed* Greenwich time thus ascertained; but on reducing two examples, first by the common, and then by Bessel's method, the discrepancy in the two results, in one case was (−) 15″, in the other (+) 5″: though the difference between the theoretical and observed distances in the first case was 1′ 9″(24″), in the other 1′ 37″(32″), both in defect. It is also to be noted that Bessel introduces a correction for the earth's double centre, and eliminates the contraction of the Moon's semi-diameter by making the limb, not the centre, of the Moon, and also of the Sun, his point of departure in computing the apparent distance. It has, no doubt, generally been taken for granted that the errors occurring in practice have always been errors of observation; the late Mr Godfray, who frequently discussed the question with me, always expressed himself entirely satisfied with the existing methods of computation, and no one will question his having been quite competent to pronounce on the question. But on the other hand, the results hitherto obtained have never, as far as I know, been comparable

[1] I refer to the folio volume of *Tables for correcting the Apparent Distance of the Moon and a Star from the effects of Parallax and Refraction, published by Order of the Commissioners of Longitude* published at Cambridge in 1772. They are said in the preface to have been computed by Dr Maskelyne, then Astronomer Royal, Mr Parkinson, Senior Wrangler in 1769, and a Mr Williams of Christ's College; and were edited by Mr Shepherd, Plumian Professor, 1760—1796. They are extremely long and must have involved a vast amount of labour: but it does not quite appear on what principles the computations were made, though I imagine that they have been the basis of the various concise methods which have been published since.

in precision with those obtained from another simple method, and, one somewhat similar in the amount of calculation needed, viz. the Occultations of planets, or fixed stars, by the Moon, as I pointed out in my former paper (*Cambridge Philosophical Proceedings*, II. 418); and, considering that more than a century has passed since the question was thoroughly taken in hand, it seems not unreasonable to suggest that it might be re-opened with advantage. It is certainly, with the chronometers of the present day, not of great practical importance, local time being always obtainable with great precision; but it is generally available on three out of four days in any month, and is, in itself, so neat and scientific, that it almost deserves to be practised for its own sake. It may be remembered, that the inventor of the formula which I have employed—which is, probably the best one available; at any rate, it is stated to be perfectly general, and to require no distinction of cases—also devised a circle specially for the purpose of making this observation; in it the limbs supporting the telescope and the mirrors are both moveable on the centre, so that the measurement commences from any point whatever in the graduated circle, and reckons continuously onwards until the series of observations is finished (so as to avoid any *constant* or *casual* error in the graduation of the circle); yet we read in Herschel's Astronomy, that "the abstract beauty and advantage of this principle seem to be counterbalanced in practice by some unknown cause, which probably must be sought for in imperfect clamping;" may it not be retorted, that we are risking our reputation as theoretical astronomers, if we always charge the blame on those who construct our instruments?

To myself it seems that the problem is simply one of Spherical Trigonometry, but at the same time somewhat complex; and not quite so straightforward as has hitherto been assumed. The situation of the observed luminaries towards the meridian, and the difference between their geocentric and geographical altitudes, are also the most hopeful points on which to discover a new correction: the former, on account of the observed errors, and the latter because it affects the position of bodies so differently at different points of the azimuth.

It is to be hoped that if these two difficulties be thoroughly reconsidered, together with any others which may suggest themselves to experienced mathematicians, means may be found of introducing a new correction of the observed distance which will give a really accurate result.

*Postscript.* Subjoined is an example of an observation worked out after Bessel's method; this is not ordinarily given in treatises bearing on the subject, but the theory will be found explained at

length in his *Astronomische Untersuchungen*, Vol. II. pp. 266—307: it was originally printed in the *Astron. Nachrichten* for 1832. It will be remembered that the problem in this case is to compute what the apparent distance of the *limbs* of the Sun and Moon will be at a given Paris time. Bessel, in his own paper, gives an example of Tables constructed to intervals of three hours, and interpolates the *data* for a time intervening between two given times; I myself have found it more convenient to compute the *data* for a certain time directly.

March 30, 1876.

☽ and ☉ $a = 4^h\ 41^m\ 39^s{\cdot}1$ $\quad A = 0\ 37\ 36{\cdot}4$ $\quad \delta\ 27^\circ\ 20'\ 13''$ $\quad \Delta\ 4\ 3\ 18$ $\quad \pi\ 59'\ 27''$ $\quad \pi'\ \ldots\ 8''{\cdot}86$

P.M.T. $2^h\ 26^m\ 38^s$ $\quad a - A = 4\ 4\ 2{\cdot}7$ $\quad \epsilon = {\cdot}0816$

L.M.T. $2\ 17\ 47$

$$\begin{aligned}
\cos(a - A) &= 9{\cdot}6854174 \\
\sin\delta = 9{\cdot}6620231 \qquad \cos\delta &= 9{\cdot}9485701 \qquad a = {\cdot}0324735 \\
\sin\Delta = 8{\cdot}8495056 \qquad \cos\Delta &= 9{\cdot}9989114 \qquad b = {\cdot}4294364 \\
&\qquad\qquad (a+b) = {\cdot}4619099 \\
\log(a) = 8{\cdot}5115287 \qquad \log(b) &= 9{\cdot}6328989 \qquad \log(a+b) = 9{\cdot}6645573 = \log\cos 62^\circ\ 29'\ 22''\tfrac{1}{2} = d
\end{aligned}$$

$$\begin{aligned}
d &= 62^\circ\ 29'\ 22\tfrac{1}{2}'' \\
h &= \cdot\ 16\ \ 2\tfrac{1}{2} \\
(d - h) &= 62^\circ\ 13'\ 20''
\end{aligned}$$

$$\begin{aligned}
l\cos\delta &= 9{\cdot}9485701 \\
l\sin(a - A) &= 9{\cdot}9418665 \\
&\phantom{=}\ 9{\cdot}8904366 \\
l\sin d &= 9{\cdot}9478879 \\
l\sin\psi &= 9{\cdot}9425487 \\
\psi &= 61^\circ\ 10'\ 27''
\end{aligned}$$

$$\begin{aligned}
(d - h) &= 62^\circ\ 13'\ 20'' \\
\rho &\qquad\ \ 16\ \ 13\tfrac{1}{2} \\
(d - h - \rho) &= 61^\circ\ 57'\ 6\tfrac{1}{2}'' \\
(-) &\qquad\qquad\ 3(-) \\
d_1 &= 61^\circ\ 57'\ 9\tfrac{1}{2}''
\end{aligned}$$

$$\begin{aligned}
l\sin\pi' &= 0{\cdot}9474337 \\
l\sin(d - h) &= 9{\cdot}9468264 \\
&\phantom{=}\ 0{\cdot}8942601 \\
l\sin\pi &= 8{\cdot}2378563 \\
&\phantom{=}\ 2{\cdot}6564038 \\
&= \log 453\tfrac{1}{2}'' \\
e &= \ 7'\ 33\tfrac{1}{2}'' \\
h &= 16\ \ 2\tfrac{1}{2} \\
h - e &= \ 8'\ 29''
\end{aligned}$$

$$\begin{aligned}
\log(h - e) &= 2{\cdot}7067178 \\
l\sin\psi &= 9{\cdot}9425487 \\
l\sec\Delta &= 0{\cdot}0010886 \\
&\phantom{=}\ 2{\cdot}6503551 \\
&= \log \qquad 29^s{\cdot}8 \\
A &= \ 0\ \ 37\ \ 36{\cdot}4 \\
\bar{A} &= \ 0\ \ 38^m\ \ 6^s{\cdot}2
\end{aligned}$$

$$\begin{aligned}
\log(h - e) &= 2{\cdot}7067178 \\
l\sin\psi &= 9{\cdot}9425487 \\
l\tan\Delta &= 8{\cdot}8505942 \\
&\phantom{=}\ 1{\cdot}4998607 \\
&= \log 31\tfrac{1}{2}'' \\
\psi &= 61^\circ\ 10'\ 27'' \\
&\qquad\qquad\ 31\tfrac{1}{2} \\
Q &= 61^\circ\ 10'\ 58\tfrac{1}{2}''
\end{aligned}$$

$$\begin{aligned}
l(h - e) &= 2{\cdot}7067178 \\
l\cos\psi &= 9{\cdot}6831810 \\
&\phantom{=}\ 2{\cdot}3898988 \\
&= \log 246''(= 4'\ 6'') \\
\Delta &= 4^\circ\ 3'\ 18'' \\
&\qquad\ \ 4\ \ \ 6 \\
\bar{\Delta} &= 4^\circ\ \ 7'\ \ 24''
\end{aligned}$$

$$\begin{aligned}
\sin\bar{\Delta} &= 8{\cdot}8567509 \\
\sin x &= 9{\cdot}9462479 \\
\log(a) &= 8{\cdot}8029988 \\[4pt]
\cos\bar{\Delta} &= 9{\cdot}9988744 \\
\cos x &= 9{\cdot}6704987 \\
\cos Q &= 9{\cdot}6830605 \\
\log(b) &= 9{\cdot}3524336 \\
a &= \ {\cdot}0635329 \\
b &= \ {\cdot}2251301 \\
(a - b) &= \ {\cdot}1615972\ (-) \\
\log(a - b) &= 9{\cdot}2084339\ (-)
\end{aligned}$$

$$\begin{aligned}
\log\omega &= 5{\cdot}3144255 \\
\log\epsilon^2 &= \bar{3}{\cdot}8233804 \\
l\sin\pi &= 8{\cdot}2378563 \\
l(a - b) &= 9{\cdot}2084339\ (-) \\
l\sin\phi &= 9{\cdot}8977286 \\
&\phantom{=}\ 0{\cdot}4818247\ (-) \\
&= \log 3''\ (-)
\end{aligned}$$

| | |
|---|---|
| $(d-h-\rho)$ $61^0\ 57'\ 6\frac{1}{2}''$ | $A-\bar{A}=$ $29^s.8(-)$ |
| $e$ $7\ 33\frac{1}{2}$ | L. App. T. $=$ $2\ 13\ 24^7$ |
| $(d-h-\rho+e)=62^0\ 4'\ 40''$ | $t=$ $2^h\ 12^m\ 55^s$ |
| $=x$ | |
| $l\sin\pi=8{\cdot}2378563$ | |
| corr. for $\phi$ $\cdot0009040$ | |
| $l\sin\pi_1=8{\cdot}2387603$ | |

I.

| | | | | | |
|---|---|---|---|---|---|
| $l\cos\phi$ | 9·7873675 | comp. $lf$ | 0·0259877 | | |
| $l\sin t$ | 9·7387719 | $la$ | 9·5261394 | | |
| $l\cos t$ | 9·9224584 | $l\operatorname{cosec}(F+\bar{\Delta})$ | 0·2195524 | | |
| $lf\sin F$ | 9·7098259 | $l\tan Z\sin q$ | 9·7716795 | | |
| $lf\cos F$ | 9·8977286 | $l\tan Z\cos q$ | 0·1213399 | | |
| $l\tan F$ | 9·8120973 | $l\tan q$ | 9·6503396 | $P_1=Q-q$ | |
| | | | | | $=61^0\ 10'\ 58\frac{1}{2}''$ |
| $F=$ | $32^0\ 58'\ 29''$ | $q=$ | $24^0\ 5'\ 10\frac{1}{2}''$ | | $24\ \ 5\ 10\frac{1}{2}$ |
| $\bar{\Delta}$... | $4\ \ 7\ 24$ | | | | $37^0\ \ 5'\ 48''$ |
| $(F+\bar{\Delta})$... | $37^0\ \ 5'\ 53''$ | $l\sin q$ | 9·6107788 | | |
| $l\sin F$ | 9·7358136 | $l\tan Z$ | 0·1609007 | | |
| | | $Z=$ | $55^0\ 22'\ 44''$ | | |

II.

| | | | | | |
|---|---|---|---|---|---|
| | | $\sin(d_1+e_1)$ | ·8835909 | | $d_1=61^0\ 57'\ 9\frac{1}{2}''$ |
| $l\sin\pi_1$ | 8·2387603 | (2) | ·0113741 | | $e=e_1=\ \ 7\ 33\frac{1}{2}$ |
| $l\cos Z$ | 9·7544607 | $r'\sin(d''+e')\cos(P'-P_1)$ | ·8722168 | | $(d_1+e_1)=62^0\ \ 4'\ 43''$ |
| $l\sin Z$ | 9·9153614 | log do. | 9·9406244 | | |
| (1) | 7·9932210 | (3) | 7·9345554 | | |
| $l\sin\pi_1\sin Z$ | 8·1541217 | $l\tan(P'-P_1)$ | 7·9939310 | | |
| $l\cos P_1$ | 9·9017955 | $(P'-P_1)$ | $0^0\ 33'\ 54''$ | | |
| $l\sin P_1$ | 9·7804337 | $l\cos(P'-P_1)$ | 9·9999789 | $le_1$ | 2·6564038 |
| (2) | 8·0559172 | $lr'\sin(d''+e')$ | 9·9406455.................... | | 9·9406455 |
| (3) | 7·9345554 | $lr'\cos(d''+e')$ | 9·6612585 | $l\operatorname{cosec}(d''+e')$ | 0·0529598 |
| $\cos(d_1+e_1)$ | ·4682597 | $l\tan(d''+e')$ | 0·2793870 | $le'$ | 2·6500091 |
| (1) | ·0098451 | | | | $=\log 447''(=7'\ 27'')$ |
| $r''\cos(d''+e')$ | ·4584146 | $(d''+e')$ | $=62^0\ 16'\ 33''$ | | |
| | | $e'$ | $=\ \ 7\ 27$ | | |
| | | $d''$ | $=62^0\ \ 9'\ \ 6''$ | | |

III.

| | | | | | |
|---|---|---|---|---|---|
| $l\tan Z$ | 0·1609007 | $l.k.$ | 1·76134 | $lK$ | 1·76047 |
| $l\cos P'$ | 9·8985236 | $l\tan(d''+e'-H)$ | 9·37593 | $l\tan(H-e')$ | 0·05753 |
| $l\tan H$ | 0·0594243 | | 1·13727 | | 1·81800 |
| | | | $=l\ 13''{\cdot}7$ | | $=l\ \ 65''{\cdot}8$ |
| $H$ | $=48^0\ 54'\ 28''$ | | | | $=\ \ 1'\ \ 6''$ |
| $(d''+e'-H)$ | $=13^0\ 22'\ \ 5''$ | | | | $14''$ |
| $H-e'$ | $=48^0\ 47'\ \ 1''$ | | | Refraction | $1'\ 20''(-)$ |
| $l\cos Z$ | 9·7544607 | | | $d''$ | $=62^0\ 9\ 6$ |
| $l\sec H$ | 0·1822542 | | | $D$ | $62^0\ 7'\ 46''$ |
| $l\cos(d''+e'-H)$ | 9·9880704 | | | | |
| $l\cos z$ | 9·9247853 | | | | |
| | | | | | |
| $z$ | $=32^0\ 45'\ 23''$ | | | | |

We thus obtain:

| | |
|---|---|
| Theoretical app. dist. of nearest limbs of ☽ and ☉ .............. | 62° 7′ 46″ |
| Observed do. do. as per note-book .............................. | 62° 6′ 5″ |
| Error of observation (in defect)... | 1′ 41″ |

On the other hand, the formula employed, p. 169, gave

| | |
|---|---|
| P. M. T. (by Observation)......... | $2^h\ 23^m\ 50^s$ |
| Ditto (by Chronometer)....... | 2 26 38 |
| Error in time ....................... | $2^h\ 48^s$ (=1 33″ arc) |

also in defect, as the Moon was in the first quarter, and therefore *increasing* her distance.

This example was selected quite at random; except in this respect, that Bessel's system would have required a little more care in application had the Moon been approaching the Sun, and the simpler case of the two was thought preferable as an example.

## ANNUAL GENERAL MEETING.

*October* 28, 1878.

PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

The following were elected Officers and new members of Council for the ensuing year:—

*President.*

Professor Liveing.

*Vice-Presidents.*

Professor Stokes.
Professor Newton.
Professor Clerk Maxwell.

*Treasurer.*

Dr J. B. Pearson.

*Secretaries.*

Mr J. W. Clark.
Mr Coutts Trotter.
Mr J. W. L. Glaisher.

*New Members of Council.*

Professor Humphry.
Professor Cayley.
Mr W. M. Hicks.

The following communications were made to the Society :—

(1) Professor Cayley, *On the transformation of co-ordinates.*

The formulæ for the transformation between two sets of oblique co-ordinates assume a very elegant form when presented in the notation of matrices. I call to mind that a matrix denotes a system of quantities arranged in a square form

$$\left(\begin{array}{ccc} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{array}\right),$$

see my "Memoir on the Theory of Matrices," *Phil. Trans.* t. CXLVII. (1857), pp. 273—312; moreover $(\alpha, \beta, \gamma \between x, y, z)$ denotes $\iota x + \beta y + \gamma z$, and so

$$\left(\begin{array}{ccc} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{array} \between x, y, z\right)$$

denotes $(\alpha x + \beta y + \gamma z,\ \alpha' x + \beta' y + \gamma' z,\ \alpha'' x + \beta'' y + \gamma'' z)$,

and again

$$\left(\begin{array}{ccc} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{array} \between x, y, z \between \xi, \eta, \zeta\right) \text{ denotes } \begin{array}{l} \xi\,(\alpha\, x + \beta\, y + \gamma\, z) \\ +\,\eta\,(\alpha' x + \beta' y + \gamma' z) \\ +\,\zeta\,(\alpha'' x + \beta'' y + \gamma z''). \end{array}$$

Consequently

$$\left(\begin{array}{ccc} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{array} \between x, y, z \between \zeta, \eta, \zeta\right) = \left(\begin{array}{ccc} \alpha, & \alpha', & \alpha'' \\ \beta, & \beta', & \beta'' \\ \gamma, & \gamma', & \gamma'' \end{array} \between \xi, \eta, \zeta \between x, y, z\right).$$

In the case of a symmetrical matrix

$$\left(\begin{array}{ccc} a, & h, & g \\ h, & b, & f \\ g, & f, & c \end{array}\right),$$

$$\left(\begin{array}{ccc} a, & h, & g \\ h, & b, & f \\ g, & f, & c \end{array} \between x, y, z \between \xi, \eta, \zeta\right), \; = \left(\begin{array}{ccc} a, & h, & g \\ h, & b, & f \\ g, & f, & c \end{array} \between \xi, \eta, \zeta \between x, y, z\right),$$

is also written

$$(a, b, c, f, g, h \between x, y, z \between \xi, \eta, \zeta), \text{ or } (a, \ldots \between \xi, \eta, \zeta \between x, y, z),$$

and in particular, if

$$(\xi, \eta, \zeta) = (x, y, z),$$

then

$$\left(\begin{array}{ccc} a, & h, & g \\ h, & b, & f \\ g, & f, & c \end{array} \between x, y, z\right)^2 \text{ is written } (a, b, c, f, g, h \between x, y, z)^2.$$

Two matrices are compounded together according to the law

$$\left(\begin{array}{ccc} a, & b, & c \\ a', & b', & c' \\ a'', & b'', & c'' \end{array} \between \begin{array}{|ccc|} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{array}\right) = \begin{array}{c|ccc} & (\alpha, \alpha', \alpha''), & (\beta, \beta', \beta''), & (\gamma, \gamma', \gamma''), \\ \hline (a, b, c) & \text{"} & \text{"} & \text{"} \\ (a', b', c') & \text{"} & \text{"} & \text{"} \\ (a'', b'', c'') & \text{"} & \text{"} & \text{"} \end{array}$$

viz., in the compound matrix, the top-line is

$$(a, b, c \between \alpha, \alpha', \alpha''), \quad (a, b, c \between \beta, \beta', \beta''), \quad (a, b, c \between \gamma', \gamma'', \gamma''),$$

and the other two lines are the like functions with $(a', b', c')$ and $(a'', b'', c'')$ respectively in the place of $(a, b, c)$.

The inverse matrix is the matrix the terms of which are the minors of the determinant formed out of the original matrix, each minor divided by this determinant, viz.,

$$\left(\begin{array}{ccc} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{array}\right)^{-1} = \frac{1}{\nabla}\left(\begin{array}{ccc} \beta'\gamma'' - \beta''\gamma', & \beta''\gamma - \beta\gamma'', & \beta\gamma' - \beta'\gamma \\ \gamma'\alpha'' - \gamma''\alpha', & \gamma''\alpha - \gamma\alpha'', & \gamma\alpha' - \gamma'\alpha \\ \gamma''\alpha - \gamma\alpha'', & \alpha''\beta - \alpha\beta'', & \alpha\beta' - \alpha'\beta \end{array}\right),$$

where $\nabla$ is the determinant

$$\left|\begin{array}{ccc} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{array}\right|.$$

Coming now to the question of transformation, write

$$\begin{array}{c|ccc:ccc}
 & x & y & z & x_1 & y_1 & z_1 \\ \hline
x & 1 & \nu & \mu & \alpha & \alpha' & \alpha'' \\
y & \nu & 1 & \lambda & \beta & \beta' & \beta'' \\
z & \mu & \lambda & 1 & \gamma & \gamma' & \gamma'' \\ \hdashline
x_1 & \alpha & \beta & \gamma & 1 & \nu_1 & \mu_1 \\
y_1 & \alpha' & \beta' & \gamma' & \nu_1 & 1 & \lambda_1 \\
z_1 & \alpha'' & \beta'' & \gamma'' & \mu_1 & \lambda_1 & 1
\end{array}
\quad = \quad
\begin{array}{c|c:c}
 & x\ y\ z & x_1\ y_1\ z_1 \\ \hline
\begin{matrix} x \\ y \\ z \end{matrix} & \Omega & W \\ \hdashline
\begin{matrix} x_1 \\ y_1 \\ z_1 \end{matrix} & V & \Omega_1
\end{array}$$

viz., the axes of $x$, $y$, $z$ are inclined to each other at angles the cosines whereof are $\lambda$, $\mu$, $\nu$: those of $x_1$, $y_1$, $z_1$ are inclined to each other at angles the cosines whereof are $\lambda_1$, $\mu_1$, $\nu_1$: and the cosines of the inclinations of the two sets of axes to each other are $\alpha, \beta, \gamma$; $\alpha', \beta', \gamma'$; $\alpha'', \beta'', \gamma''$; as is more clearly indicated in the diagram, the top-line showing that

cosine-inclinations of $x$ to $x, y, z, x_1, y_1, z_1$

are $1, \nu, \mu, \alpha, \alpha', \alpha''$ respectively,

and the like for the other lines of the diagram. The letters $\Omega$, $\Omega_1$, $V$, $W$ are used to denote matrices, viz., as appearing by the diagram, these are

$$\left(\begin{array}{ccc} 1, & \nu, & \mu \\ \nu, & 1, & \lambda \\ \mu, & \lambda, & 1 \end{array}\right), \quad \left(\begin{array}{ccc} 1, & \nu_1, & \mu_1 \\ \nu_1, & 1, & \lambda_1 \\ \mu_1, & \lambda_1, & 1 \end{array}\right), \quad \left(\begin{array}{ccc} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{array}\right), \quad \left(\begin{array}{ccc} \alpha, & \alpha', & \alpha'' \\ \beta, & \beta', & \beta'' \\ \gamma, & \gamma', & \gamma'' \end{array}\right)$$

respectively.

The coordinates $(x, y, z)$ and $(x_1, y_1, z_1)$ form each set a broken line extending from the origin to the point; hence projecting on the axes of $x$, $y$, $z$ and on those of $x_1$, $y_1$, $z_1$ respectively, we have two sets, each of three equations, which may be written

$$(\Omega \between x,\ y,\ z) = (W \between x_1,\ y_1,\ z_1),$$
$$(V \between x,\ y,\ z) = (\Omega_1 \between x_1,\ y_1,\ z_1);$$

and where of course each set implies the other set.

We have

$$(x,\ y,\ z) = (\Omega^{-1} W \between x_1,\ y_1,\ z_1), \ = (V^{-1}\Omega_1 \between x_1,\ y_1,\ z_1),$$
$$(x_1,\ y_1,\ z_1) = (W^{-1}\Omega \between x,\ y,\ z), \ = (\Omega_1^{-1} V \between x,\ y,\ z),$$

the first giving in two forms $(x, y, z)$ as linear functions of $(x_1, y_1, z_1)$, and the second giving in two forms $(x_1, y_1, z_1)$ as linear

functions of $(x, y, z)$; comparing the two forms for each set, we have

$$\Omega^{-1}W = V^{-1}\Omega_1, \quad W^{-1}\Omega = \Omega_1^{-1}V,$$

or, what is the same thing,

$$V\Omega^{-1}W = \Omega_1, \qquad W\Omega_1^{-1}V = \Omega,$$

where in each equation the two sides are matrices which must be equal term by term to each other, but the matrices being symmetrical the equation thus gives (not nine but only) six equations. Writing

$$(\mathrm{a},\ \mathrm{b},\ \mathrm{c},\ \mathrm{f},\ \mathrm{g},\ \mathrm{h}) = (1-\lambda^2,\ 1-\mu^2,\ 1-\nu^2,\ \mu\nu-\lambda,\ \nu\lambda-\mu,\ \lambda\mu-\nu),$$

and

$$K = 1-\lambda^2-\mu^2-\nu^2+2\lambda\mu\nu,$$

we have

$$\Omega^{-1} = \frac{1}{K}\left(\begin{array}{ccc} \mathrm{a}, & \mathrm{h}, & \mathrm{g} \\ \mathrm{h}, & \mathrm{b}, & \mathrm{f} \\ \mathrm{g}, & \mathrm{f}, & \mathrm{c} \end{array}\right),$$

and the first equation written in the form

$$V\left(\begin{array}{ccc} \mathrm{a}, & \mathrm{h}, & \mathrm{g} \\ \mathrm{h}, & \mathrm{b}, & \mathrm{f} \\ \mathrm{g}, & \mathrm{f}, & \mathrm{c} \end{array}\right)W = K\Omega_1,$$

in fact denotes the six equations

$$\begin{array}{lll}
(\mathrm{a},\ \mathrm{b},\ \mathrm{c},\ \mathrm{f},\ \mathrm{g},\ \mathrm{h}) & (\alpha\ ,\ \beta\ ,\ \gamma\ )^2 & = K\ , \\
\quad " & (\alpha',\ \beta',\ \gamma')^2 & = K\ , \\
\quad " & (\alpha'',\ \beta'',\ \gamma'')^2 & = K\ , \\
\quad " & (\alpha',\ \beta',\ \gamma')(\alpha'',\ \beta'',\ \gamma'') & = K\lambda_1, \\
\quad " & (\alpha'',\ \beta'',\ \gamma'')(\alpha\ ,\ \beta\ ,\ \gamma\ ) & = K\mu_1, \\
\quad " & (\alpha\ ,\ \beta\ ,\ \gamma\ )(\alpha',\ \beta',\ \gamma') & = K\nu_1.
\end{array}$$

And similarly writing

$$(\mathrm{a}_1,\ \mathrm{b}_1,\ \mathrm{c}_1,\ \mathrm{f}_1,\ \mathrm{g}_1,\ \mathrm{h}_1) = (1-\lambda_1^2,\ 1-\mu_1^2,\ 1-\nu_1^2,\ \mu_1\nu_1-\lambda_1,\ \nu_1\lambda_1-\mu_1,\ \lambda_1\mu_1-\nu_1),$$

and

$$K_1 = 1-\lambda_1^2-\mu_1^2-\nu_1^2+2\lambda_1\mu_1\nu_1,$$

then

$$\Omega_1^{-1} = \frac{1}{\kappa_1}\left(\begin{array}{ccc} \mathrm{a}_1, & \mathrm{h}_1, & \mathrm{g}_1 \\ \mathrm{h}_1, & \mathrm{b}_1, & \mathrm{f}_1 \\ \mathrm{g}_1, & \mathrm{f}_1, & \mathrm{c}_1 \end{array}\right);$$

and the second equation written in the form

$$W\left(\begin{array}{ccc} \mathrm{a}_1, & \mathrm{h}_1, & \mathrm{g}_1 \\ \mathrm{h}_1, & \mathrm{b}_1, & \mathrm{f}_1 \\ \mathrm{g}_1, & \mathrm{f}_1, & \mathrm{c}_1 \end{array}\right)V = \kappa_1\Omega,$$

in fact denotes the six equations

$$\begin{array}{lll} (\mathrm{a}_1,\ \mathrm{b}_1,\ \mathrm{c}_1,\ \mathrm{f}_1,\ \mathrm{g}_1,\ \mathrm{h}_1 \between \alpha,\ \alpha',\ \alpha'')^2 & & = K_1\ , \\ \quad ,, & (\beta,\ \beta',\ \beta'')^2 & = K_1\ , \\ \quad ,, & (\gamma,\ \gamma',\ \gamma'')^2 & = K_1\ , \\ \quad ,, & (\beta,\ \beta',\ \beta'' \between \gamma,\ \gamma',\ \gamma'') & = K_1\lambda_1, \\ \quad ,, & (\gamma,\ \gamma',\ \gamma'' \between \alpha,\ \alpha',\ \alpha'') & = K_1\mu_1, \\ \quad ,, & (\alpha,\ \alpha',\ \alpha'' \between \beta,\ \beta',\ \beta'') & = K_1\nu_1; \end{array}$$

the two sets each of six equations being in fact equivalent to a single set of six equations, and serving to express the relations between the nine cosines $(\alpha, \beta, \gamma, \alpha', \beta', \gamma', \alpha'', \beta'', \gamma'')$, and the cosines $(\lambda, \mu, \nu)$ and $(\lambda_1, \mu_1, \nu_1)$. Observe that the nine cosines are *not* (as in the rectangular transformation) the coefficients of transformation between the two sets of coordinates.

From the original linear relations between the coordinates, multiplying the equations of the first set by $x, y, z$ and adding, and again multiplying the equations of the second set by $(x_1, y_1, z_1)$ and adding, we have

$$(\Omega \between x\ , y\ , z\ )^2 = (W \between x_1, y_1, z_1 \between x\ , y\ , z\ ),$$

$$(\Omega \between_1 x_1, y_1, z_1)^2 = (V \between x\ , y\ , z \between x_1, y_1, z_1).$$

But $(W \between x_1, y_1, z_1 \between x, y, z)$ and $(V \between x, y, z \between x_1, y_1, z_1)$ denote one and the same function; hence

$$(\Omega \between x,\ y,\ z)^2 = (\Omega_1 \between x_1,\ y_1,\ z_1)^2,$$

that is,

$$(1,\ 1,\ 1,\ \lambda,\ \mu,\ \nu \between x,\ y,\ z)^2 = (1,\ 1,\ 1,\ \lambda_1,\ \mu_1,\ \nu_1 \between x_1,\ y_1,\ z_1)^2,$$

or the linear relations between $(x, y, z)$ and $(x_1, y_1, z_1)$ are such as to transform one of these quadric functions into the other: the two quadrics in fact denote the squared distance from the origin expressed in terms of the coordinates $(x, y, z)$ and $(x_1, y_1, z_1)$ respectively.

Since the nine cosines are connected by six equations, there should exist values containing three arbitrary constants, and satisfying these equations identically: but by what just precedes, it appears that the problem to determine these values is in fact that of finding the linear transformation between two given quadric functions: the problem of the linear transformation of a quadric function into itself has an elegant solution; but it would seem that this is not the case for the transformation between two different functions.

The foregoing equation

$$K = (\mathrm{a},\ \mathrm{b},\ \mathrm{c},\ \mathrm{f},\ \mathrm{g},\ \mathrm{h} \between \alpha,\ \beta,\ \gamma)^2,$$

is a relation between $\lambda, \mu, \nu$, the cosines of the sides of a spherical triangle, and $(\alpha, \beta, \gamma)$ the cosines of the distances of a point $P$ from the three vertices: it can be at once verified by means of the relation $A + B + C = 2\pi$, and thence

$$1 - \cos^2 A - \cos^2 B - \cos^2 C + 2 \cos A \cos B \cos C = 0,$$

which connects the angles $A, B, C$ which the sides subtend at $P$: writing $a, b, c$ for $\lambda, \mu, \nu$, and $f, g, h$ for $\alpha, \beta, \gamma$, the relation is

$$1 - a^2 - b^2 - c^2 + 2abc = (1 - a^2) f^2 + (1 - b^2) g^2 + (1 - c^2) h^2 \\ + 2 (bc - a) gh + 2 (ca - b) hf + 2 (ab - c) fg,$$

viz., this is

$$1 - a^2 - b^2 - c^2 - f^2 - g^2 - h^2 + 2abc + 2agh + 2bhf + 2cfg \\ - a^2f^2 - b^2g^2 - c^2h^2 + 2bcgh + 2cahf + 2abfg = 0;$$

where $(a, b, c, f, g, h)$ are the cosines of the sides of a spherical quadrangle; $(a, b, c)$, $(a, h, g)$, $(h, b, f)$, $(g, f, c)$ belong respectively to sides forming a triangle, and the remaining sides $(f, g, h)$, $(b, c, f)$, $(c, a, g)$, $(a, b, h)$ are sides meeting in a vertex.

The equation

$$K\nu_1 = (\mathrm{a},\ \mathrm{b},\ \mathrm{c},\ \mathrm{f},\ \mathrm{g},\ \mathrm{h} \between \alpha,\ \beta,\ \gamma)(\alpha',\ \beta',\ \gamma')$$

is a relation between $\lambda, \mu, \nu$, the cosines of the sides of a spherical triangle; $\alpha, \beta, \gamma$, the cosines of the distances of a point $P$ from the three vertices; $\alpha', \beta', \gamma'$, the cosines of the distances of a point $Q$ from the three vertices; and $\nu_1$, the cosine of the distance $PQ$.

Drawing a figure, it is at once seen that

$$\nu_1 = \alpha\alpha' + \sqrt{1-\alpha^2}\sqrt{1-\alpha'^2} \,.\, \cos(\theta - \theta');$$

where $\cos\theta = \dfrac{\beta - \alpha\nu}{\sqrt{1-\alpha^2}\sqrt{1-\nu^2}}$, and therefore $\sin\theta = \dfrac{\sqrt{\nabla}}{\sqrt{1-\alpha^2}\sqrt{1-\nu^2}}$,

$\cos\theta' = \dfrac{\beta' - \alpha\nu'}{\sqrt{1-\alpha'^2}\sqrt{1-\nu^2}}$ " $\sin\theta' = \dfrac{\sqrt{\nabla'}}{\sqrt{1-\alpha'^2}\sqrt{1-\nu^2}}$,

the values of $\nabla$, $\nabla'$,

$$\nabla = 1 - \alpha^2 - \beta^2 - \nu^2 + 2\alpha\beta\nu,$$
$$\nabla' = 1 - \alpha'^2 - \beta'^2 - \nu^2 + 2\alpha'\beta'\nu;$$

the resulting value of $\nu_1$ is therefore

$$\nu_1 = \alpha\alpha' + \frac{1}{1-\nu^2}\{(\beta - \alpha\nu)(\beta' - \alpha'\nu) + \sqrt{\nabla\nabla'}\}.$$

The equations

$$\kappa = (\mathrm{a,\ b,\ c,\ f,\ g,\ h})(\alpha,\ \beta,\ \gamma)^2, \quad \kappa = (\mathrm{a},\ \ldots)(\alpha',\ \beta',\ \gamma')^2$$

give
$$(\mathrm{g}\alpha + \mathrm{f}\beta + \mathrm{c}\gamma)^2 = K\nabla,$$
$$(\mathrm{g}\alpha' + \mathrm{f}\beta' + \mathrm{c}\gamma')^2 = K\nabla':$$

and we therefore have

$$(\mathrm{g}\alpha + \mathrm{f}\beta + \mathrm{c}\gamma)(\mathrm{g}\alpha' + \mathrm{f}\beta' + \mathrm{c}\gamma') = K\sqrt{\nabla\nabla'};$$

recollecting that $1 - \nu^2 = \mathrm{c}$, the formula thus is

$$\nu_1 = \alpha\alpha' + \frac{1}{\mathrm{c}}\left\{(\beta - \alpha\nu)(\beta' - \alpha\nu') + \frac{1}{K}(\mathrm{g}\alpha + \mathrm{f}\beta + \mathrm{c}\gamma)(\mathrm{g}\alpha' + \mathrm{f}\beta' + \mathrm{c}\gamma')\right\},$$

or say,

$$K\nu_1 = K\alpha\alpha' + \frac{1}{\mathrm{c}}\{K(\beta - \alpha\nu)(\beta' - \alpha'\nu) + (\mathrm{g}\alpha + \mathrm{f}\beta)(\mathrm{g}\alpha' + \mathrm{f}\beta')\}$$
$$+ \mathrm{g}(\alpha\gamma' + \alpha'\gamma) + \mathrm{f}(\beta\gamma' + \beta'\gamma) + \mathrm{c}\gamma\gamma'.$$

The sum of the first and second terms is readily found to be

$$= \mathrm{a}\alpha\alpha' + \mathrm{b}\beta\beta' + \mathrm{h}(\alpha'\beta' + \alpha'\beta),$$

and the equation thus becomes

$$K\nu_1 = (\mathrm{a,\ b,\ c,\ f,\ g,\ h})(\alpha,\ \beta,\ \gamma)(\alpha',\ \beta',\ \gamma'),$$

as it should do.

(2) Mr J. W. L. GLAISHER, M.A., F.R.S., *On circulating decimals with special reference to Henry Goodwyn's "Table of circles" and "Tabular series of decimal quotients"* (*London*, 1818—1823).

§ 1. THE chief rules relating to the conversion of vulgar fractions into decimals are as follows, $\frac{p}{q}$ denoting throughout a vulgar fraction in its lowest terms.

§ 2. (i) *If $q$ be prime*[1] *to* 10, then $\frac{p}{q}$ is equal to a pure circulating decimal (*i.e.* is equal to a decimal which begins to circulate from the digit immediately to the right of the decimal point); and the number of digits in the period is equal to a divisor of $\phi(q)$, where $\phi(q)$ denotes the number of numbers less than $q$ and prime to it. Further, if $\frac{p}{q}$ has a period of $a$ digits ($a$ being necessarily equal to $\phi(q)$ or to a submultiple of $\phi(q)$), then every fraction which, in its lowest terms, has $q$ for denominator has a period of $a$ digits, and there are altogether $n$ periods[2], where $na = \phi(q)$.

If we define the periods that arise from the series of fractions $\frac{p}{q}$, $p$ having all values less than $q$ and prime to it, as the periods of the denominator $q$ or, more simply, as the periods of $q$; the theorem is that the denominator $\phi(q)$ has a certain number ($n$) of periods, each containing the same number ($a$) of digits, $n$ and $a$ being connected by the relation, $na = \phi(q)$.

(ii) If $q$ be prime, $\phi(q) = q - 1$ so that in this case the number of digits in the period must be equal to $q - 1$ or to a submultiple of $q - 1$. If therefore $\frac{p}{q}$ gives a period of $q - 1$ digits, $q$ must be prime; but the converse is of course not true, viz. it does not follow that if $q$ be prime the period of $\frac{p}{q}$ will contain $q - 1$ digits. Also nothing can be inferred from the fact that the number of digits in the period is a submultiple of $q - 1$, for $\phi(q)$ and $q - 1$ may have common factors. Thus for $q = 33$, $a = 2$ which is a submultiple of $q - 1$, $= 32$, and also of $\phi(q)$, $= 20$; for $q = 91$,

[1] Throughout the whole of § 2, $q$ is supposed to be prime to 10.

[2] The periods $\alpha\beta\gamma \ldots \xi$, $\beta\gamma \ldots \xi\alpha$, $\gamma \ldots \xi\alpha\beta$, &c. are all regarded as the same period, *i.e.* a period may be supposed to begin with any of the digits composing it.

$a=6$, which is a submultiple of $q-1$, $=90$, and also of $\phi(q)$, $=72$.

(iii) It is convenient to adopt the following definitions. Two digits $\alpha, \alpha'$ are complementary if $\alpha+\alpha'=9$, and two periods $\alpha\beta\ldots\xi$, $\alpha'\beta'\ldots\xi'$ are complementary if $\alpha+\alpha'=9$, $\beta+\beta'=9$, ... $\xi+\xi'=9$; but two remainders are complementary if their sum is equal to the divisor, viz. if $q$ be the divisor then $r$ and $q-r$ are complementary remainders. It is also convenient to include the dividend among the remainders.

(iv) If $p$ divided by $q$ give a quotient figure $\alpha$ and a remainder $r$, then $q-p$ divided by $q$ gives a quotient figure $9-\alpha$ and a remainder $q-r$; and it follows that if $p$ and $q-p$ be both divided by $q$, and the same number ($m$) of digits in the quotient be obtained in each case, these $m$ quotient digits are complementary, and also the corresponding remainders are complementary. If $p$ be divided by $q$ we shall ultimately arrive at a remainder equal to $p$, after which the quotient digits recur. Consider however what happens if we arrive at the remainder $q-p$. We shall then obtain digits in the quotient complementary to those already obtained until we reach the remainder $p$ when the digits begin to recur. Thus if $\alpha\beta\gamma\ldots\xi$ be the quotient (of $m$ digits) up to the point at which the complementary remainder occurs then the next $m$ figures will be $\alpha'\beta'\gamma'\ldots\xi'$, where

$$\alpha+\alpha'=9, \quad \beta+\beta'=9, \quad \ldots \quad \xi+\xi'=9.$$

The following are examples[1]:

| | | | | | |
|---|---|---|---|---|---|
| 21 ) | 1 | ( $\cdot\dot{0}$ | 13 ) | 1 | ( $\cdot\dot{0}$ |
| | 10 | 4 | | 10 | 7 |
| | 16 | 7 | | 9 | 6 |
| | 13 | 6 | | 12 | 9 |
| | 4 | 1 | | 3 | 2 |
| | 19 | $\dot{9}$ | | 4 | $\dot{3}$ |
| | 1 | | | 1 | |

In the first case the complementary remainder does not occur at all; in the second it occurs after three digits so that the period consists of six digits, and the two halves are complementary. It can be shown that if any one period of a denominator $q$ consists of two complementary portions, all the periods will also consist of two complementary portions.

[1] This mode of arranging divisions, which is that employed by Mr Goodwyn in the Appendix to the *Tabular series* at the end of the *First centenary*, 1818 (see § 5), is very convenient; the two columns contain the corresponding quotient digits and remainders, viz. 10 divided by 21 gives quotient 0 and remainder 10, 100 divided by 21 gives quotient 4 and remainder 16, &c.

(v) The periods arising from the different denominators may therefore be divided into two classes as follows: (1) If a complementary remainder does not occur in a division there will be an even number of periods, which may be arranged in pairs, each pair containing complementary periods: for example, the periods of the denominator 21 are

$$\begin{array}{c} \cdot\dot{0}4761\dot{9} \\ \cdot\dot{9}5238\dot{0} \end{array}$$

and the periods of the denominator 41 are

$$\begin{array}{cccc} \cdot\dot{0}243\dot{9} & \cdot\dot{0}487\dot{8} & \cdot\dot{0}731\dot{7} & \cdot\dot{1}463\dot{4} \\ \cdot\dot{9}756\dot{0} & \cdot\dot{9}512\dot{1} & \cdot\dot{9}268\dot{2} & \cdot\dot{8}536\dot{5} \end{array}$$

each pair of complementary periods being printed in the same column.

(2) If a complementary remainder does occur in a division, each period will contain an even number of digits and the first half and the second half will be complementary. For example, the periods of 73 are

$$\begin{array}{ccc} \cdot\dot{0}136\ 986\dot{3} & \cdot\dot{0}273\ 972\dot{6} & \cdot\dot{0}410\ 958\dot{9} \\ \cdot\dot{0}547\ 945\dot{2} & \cdot\dot{0}684\ 931\dot{5} & \cdot\dot{0}821\ 917\dot{8} \\ \cdot\dot{1}232\ 876\dot{7} & \cdot\dot{1}643\ 835\dot{6} & \cdot\dot{3}424\ 657\dot{5} \end{array}$$

and in each period the two halves are complementary.

(vi) If there be but one period of the denominator $q$, or, in other words, if the period contain $\phi(q)$ digits, then, since all the possible remainders must occur in the division, the complementary remainder must occur and therefore the period must belong to the second class, *i.e.* it must consist of two complementary portions. For example, the denominator 17 has but one period, viz. $\cdot\dot{0}5882352\ 9411764\dot{7}$, the denominator 49 has but one period containing $\phi(49)$, $=42$, digits, viz.,

$$\begin{array}{c} \cdot\dot{0}20408163265306122448 \\ 97959183673469387755\dot{1} \end{array}$$

the complementary digits of the second half of the period being printed under those of the first half.

(vii) It may be observed that when a given denominator $q$ has only one period, this period must be such that when multiplied by each of the $\phi(q)$ numbers less than $q$ and prime to it, the resulting products are the same period, though commencing at a different place. Thus the first 16 multiples of $\cdot 0588235294117647$ consist of these same figures in the same cyclical order. Similarly if the period of the denominator 49 be multiplied by the 42 numbers 1, 2, 3, 4...48 which are less than 49 and prime to it, the products reproduce the same digits in the same cyclical order.

(viii) If $q$ be a prime and the periods of $q$ each contain an even number of digits, the two halves of each period will be complementary; for example, the periods of 13 are $\dot{0}76\ 92\dot{3}$ and $\dot{1}53\ 84\dot{6}$.

(ix) With regard to the number of figures in the periods of the denominator $q$, the rules are as follows. If $q = rst\ldots$, where $r, s, t, \ldots$ are primes (so that $q$ has no squared factor), and if the periods of $r$ contain each $\rho$ digits, of $s$ contain each $\sigma$ digits, of $t$, $\tau$ digits, and so on; then the periods of $q$ will each contain $\omega$ digits, where $\omega$ is the least common multiple of $\rho, \sigma, \tau \ldots$. For example, the periods of 13 contain six digits, of 31 fifteen digits, and of 41 five digits; therefore the periods of 16523, $= 13 \times 31 \times 41$, contain 30 digits.

(x) The reasoning by which the preceding theorem is established requires that $r, s, t\ldots$ should be different primes. Suppose now that $q = r^2$, $r$ being a prime whose periods contain each $\rho$ digits; then $10^\rho - 1$ is a multiple of $r$. It does not however necessarily follow that $10^\rho - 1$ is not a multiple of $r^2$, *i.e.* the periods of $r$ may be divisible by $r$, in which case the period of $r^2$ would contain only $\rho$ digits. In the general case however in which $10^\rho - 1$ is not divisible by $r^2$, the periods of $r^2$ will contain $r\rho$ digits, and the periods of $r^k$ will contain $\rho r^{k-1}$ digits.

Thus *generally* when $q = r^k s^l t^m \ldots$, $r, s, t, \ldots$ being primes, it follows that the periods of $q$ contain $\omega$ digits, where $\omega$ is the least common multiple of $\rho^{k-1}r, \sigma^{l-1}s, \tau^{m-1}t\ldots$; the condition for the truth of this theorem being that the periods of $r, s, t, \ldots$ are not divisible by $r, s, t, \ldots$ respectively, or in other words that $10^\rho - 1$ is not divisible by $r$, $10^\sigma - 1$ is not divisible by $s$, and so on.

*Exceptions.* The case $r = 3$ is an exception to the general rule: for, since $3 = \cdot\dot{3}$, the periods of 3 are divisible by 3, and the periods of $3^2$ contain only one digit. Thus the periods of $3^k$ contain $3^{k-2}$ digits. The case $r = 487$ is also an exception, for the prime 487 has but one period which therefore consists of 486 digits, and this period is found to be divisible by 487 (but not by $487^2$). It follows therefore that the periods of $(487)^k$ contain $486\ (487)^{k-2}$ digits (see § 11).

(xi) Desmarest states that, with the exception of 3 and 487, there are no primes, up to 1000, which are divisors of their periods; so that if $r, s, t, \ldots$ be any primes less than 1000, other than 3 and 487, the number of digits in the period of $r^k s^l t^m \ldots$ is given by the general theorem, while if $r = 3$, $s = 487$, the quantities $\rho r^{k-1}$, $\sigma s^{l-1}$ are to be respectively replaced by $3^{k-2}$, $486\ (487)^{l-2}$.

(xii) If $q$ have but one period, and if this period be not divisible by $q$, then $q^k$ will have but one period, for the period of

$q^k$ will contain $(q-1)\,q^{k-1}$ digits, and $\phi(q)=(q-1)\,q^{k-1}$. Also $3^k$ $(k>1)$ will have 6 periods and $487^k$ will have 487 periods.

(xiii) Unless $q$ be a prime or a power of a prime, it must have more than one period; for there will be a single period only in the case in which the period contains $\phi(q)$ digits. Now if $q=r^k s^l t^m \ldots$ the number of figures in the period is the least common multiple of $\rho r^{k-1}$, $\sigma s^{l-1}$, $\tau t^{m-1}, \ldots$; and this cannot be equal to $\phi(q)$ unless $\rho=r-1$, $\sigma=s-1$, &c. and $r^{k-1}(r-1)$, $s^{l-1}(s-1), \ldots$ be prime to one another. But this cannot happen for $r, s, \ldots$ being primes, $r-1$, $s-1, \ldots$ must have the common factor 2; and therefore every number other than a prime or the power of a prime must have at least two periods.

§ 3. If $q$ be not prime to 10, then $q$ is either of the form $2^m 5^n$ or of the form $2^m 5^n s$, where $s$ is prime to 10. In the former case the fraction $\dfrac{p}{q}$ is equal to a terminating decimal and the number of decimal places is equal to $r$, where $r$ is the greater of the numbers $m$ and $n$.

If $q=2^m 5^n s$, then the decimal fraction equal to $\dfrac{p}{2^m 5^n s}$ will consist of $r$ non-circulating digits ($r$ being the greater of the numbers $m$ and $n$) followed by one of the periods of $s$ as circulating period. For,

$$\frac{p}{2^m 5^n s}=\frac{1}{10^r}\left\{\frac{2^{r-m}5^{r-n}p}{s}\right\}.$$

Now let the quantity in brackets $=M+\dfrac{p'}{s}$, where $M$ is an integer and $p'<s$; then the given fraction

$$=\frac{M}{10^r}+\frac{1}{10^r}\frac{p'}{s},$$

that is to say the first $r$ digits of the decimal are not periodic and are obtained by dividing $2^{r-m}5^{r-n}p$ by $s$, and then one of the periods of $s$ commences.

*Examples.*

(i) $$\frac{73}{784}=\frac{73}{2^4\times 49}=\frac{1}{10^4}\frac{73\times 5^4}{49}=\frac{1}{10^4}\left\{\frac{45625}{49}\right\}$$

$$=\frac{1}{10^4}\{931\tfrac{6}{49}\}=\cdot 0931\tfrac{6}{49}$$

$$=\cdot 0931\ \dot{1}224489795918367346938775510204081632653\dot{0}6.$$

$$\text{(ii)}\quad \frac{57}{656}=\frac{57}{2^4\times 41}=\frac{1}{10^4}\,\frac{57\times 5^4}{41}=\frac{1}{10^4}\left\{\frac{35625}{41}\right\}$$

$$=\frac{1}{10^4}\{868\tfrac{37}{41}\}=\cdot 0868\tfrac{37}{41}=\cdot 0868\ \dot{9}024\dot{3}.$$

It is scarcely necessary to remark that whatever the denominator $q$ may be, $\phi(q)$ represents the number of remainders that occur in the divisions, and also the number of proper fractions which in their lowest terms have $q$ for denominator.

§ 4. The full titles of Mr Goodwyn's works of 1823 are "A tabular series of decimal quotients for all the proper vulgar fractions of which, when in their lowest terms, neither the numerator nor the denominator is greater than 1000. London ... 1823," and "A table of the circles arising from the division of a unit, or any other whole number, by all the integers from 1 to 1024; being all the pure decimal quotients that can arise from this source. London ... 1823." The former contains 5 pages of introduction, &c. and 153 pages of tables, and the latter contains 5 pages of introduction, &c. and 118 pages of tables; the paging however ends at p. 111. The two form one volume in a copy before me; but they were also published separately as appears from the words "the Table of Circles, which is subjoined to the work, but sold, also, as a separate publication ..." which occur in the introduction to the 'Tabular series.' Mr Goodwyn's name does not appear in connexion with either work, but there is no doubt about the authorship, as reference is made to "a short specimen" which the author had published in 1818 under the title "The First Centenary of a Series of Concise and Useful Tables..."; and this specimen bears the name of Henry Goodwyn.

Mr Goodwyn's volume of 1823 contains three tables, the most important being the 'table of circles' which occupies 107 pages. It contains all the periods (or *circles*) of every denominator $q$, prime to 10, from $q=1$ to $q=1024$. The following are specimens.

**41**

$\cdot\dot{0}243\dot{9}$ $\quad\cdot\dot{0}487\dot{8}$ $\quad\cdot\dot{0}731\dot{7}$ $\quad\cdot\dot{1}463\dot{4}$

$\cdot\dot{9}756\dot{0}$ $\quad\cdot\dot{9}512\dot{1}$ $\quad\cdot\dot{9}268\dot{2}$ $\quad\cdot\dot{8}536\dot{5}$

**127**

$\cdot\dot{0}07874015748031496062$
$99212598425196850393\dot{7}$

$\cdot\dot{0}23622047244094488188$ $\quad\cdot\dot{0}70866141732283464566$
$97637795275590551181\dot{1}$ $\quad 92913385826771653543\dot{3}$

141

$$\cdot\dot{0}07092198581560283687943262411347517730496453\dot{9}$$
$$\cdot\dot{9}92907801418439716312056737588652482269503546\dot{0}$$

In this manner *all* the periods of the numbers prime to 10 up to 1024 are given; the digits in the complementary periods or half periods being always printed under the digits to which they are complementary. Thus in the case of 41 and 141 the complementary periods are printed in the second line, and in the case of 127 the second half of each period is printed under the first half.

The 'tabular series' contains the first eight (or in special cases nine or ten) digits of the decimal equivalent to every vulgar fraction, in its lowest terms, whose numerator and denominator are both not greater than 1000 from $\frac{1}{1000}$ to $\frac{99}{991}$, arranged in order of magnitude. It was Mr Goodwyn's intention[1] to have extended the table as far as $\frac{1}{2}$; this would have included all fractions which, in their lowest terms, have numerators and denominators both not greater than 1000, as the decimal values of the vulgar fractions between $\frac{1}{2}$ and 1 are obtained at once from those between 0 and $\frac{1}{2}$, by replacing each digit by its complement.

There is also at the end of the 'table of circles' a small table of two pages entitled, "A table shewing under what divisor in the preceding table of circles those are to be found that arise from any given divisor between 1 and 1024." Adopting the notation of § 3, it gives the value of $s$ corresponding to every value of $q$ from 1 to 1024, *i.e.* corresponding to $q$ the table gives the resulting factor when powers of 2 and 5 have been thrown out. If $q$ is of the form $2^m5^n$ the letter T is given as tabular result, indicating that the decimal terminates.

Taking the examples in § 3, if we require the complete decimal fraction equivalent to $\frac{73}{784}$ we enter the 'tabular series' with this number and find as tabular result $\cdot 0931\dot{1}224$. Entering the small table with 784 we obtain as tabular result 49, and entering the 'table of circles' with 49 we find the single period of 49 which has been quoted in § 1 (vi), and beginning this with the digits $\dot{1}224$

[1] It is stated in the introduction that "the table which is now submitted to public inspection is the first part of one which is intended to exhibit the decimal value of every proper fraction whose denominator is less than 1000," and at the end is printed "end of part I." This part contains all fractions whose decimal values begin with ·0, and the author's intention was that the second part should contain those whose decimal values begin with ·1; the third those whose decimals begin with ·2; the fourth with ·3, and the fifth with ·4. The publication of these other parts is stated to be dependent upon the reception which the present work met with: and no more was ever published. De Morgan, in his article on *Tables* in the English Cyclopædia, states that "Mr Goodwyn's manuscripts, an enormous mass of similar calculations, came into the possession of Dr Olinthus Gregory, and were purchased by the Royal Society at the sale of his books in 1812." Nothing however is known of them at the Royal Society.

we can add the remaining 38 digits of the period, viz. 4897...5306 as in § 3.

Similarly, entering the 'tabular series' with $\frac{57}{656}$ we find ·08689024, entering the small table with 656 we find 41; and looking among the periods of 41 in the 'table of circles' [the periods are quoted near the beginning of this section] for one containing the digits 9024 we find the complete period to be $\dot{9}024\dot{3}$, so that the required decimal is $\cdot 0868\dot{9}024\dot{3}$.

§ 5. It will thus be seen that Mr Goodwyn's tables give with great ease the complete decimal value of a vulgar fraction, when this vulgar fraction has been found in the 'tabular series'; but as the fractions in the 'tabular series' are arranged in order of magnitude it is not very easy to find a given vulgar fraction among the series of arguments. This Mr Goodwyn himself acknowledges in his introduction to the 'Tabular series' (1823) for he writes (p. iv.): "But though it is easy to find, from this Tabular Series, a fraction either *exactly* or *nearly* equivalent to a given decimal, the table does not show, with equal readiness, the decimal corresponding to a vulgar fraction. This latter object is best effected by such an arrangement as that adopted in the first of the tables contained in the 'Centenary' above referred to, under the title of 'Tables of complete Decimal Quotients.' The Table of complete Decimal Quotients was the source, indeed, from which the Tabular Series was derived; and, again, the principal source from which the Table of complete Decimal Quotients originated was 'The Table of Circles,' which was necessarily completed before either the Table of Decimal Quotients or the Tabular Series were began. But whether that table shall be printed, or the present work completed, will depend on the reception which this result of the Computer's labour may meet with from the public."

The full title of the "First centenary" is "The first centenary of a series of concise and useful tables of all the complete decimal quotients which can arise from dividing a unit, or any whole number less than each divisor, by all integers from 1 to 1024. To which is now added a tabular series of complete decimal quotients for all the proper vulgar fractions of which, when in their lowest terms, neither the numerator nor the denominator, is greater than 100: with the equivalent vulgar fractions prefixed. By Henry Goodwyn, London...1818." The work is of quarto size. The 'First centenary' contains pp. xiv. + 18: then follows the 'Tabular series' (with a separate title-page) which contains pp. vii. + 30 (pp. 17—30 forming an appendix). The 'first centenary' consists of a series of a hundred small tables, corresponding to the first hundred numbers. The following is a specimen:—

| | | |
|---|---|---|
| | **38**<br>2·19 | |
| | $\cdot\dot{7}36842105$<br>$263157894\dot{4}$ | |
| 37 | $\cdot 9\dot{7}3684$ | 1 |
| 35 | $\cdot 9\dot{2}1052$ | 3 |
| 33 | $\cdot 8\dot{6}8421$ | 5 |
| 31 | $\cdot 8\dot{1}5789$ | 7 |
| 29 | $\cdot 7\dot{6}3157$ | 9 |
| 27 | $\cdot 7\dot{1}0526$ | 11 |
| 25 | $\cdot 6\dot{5}7894$ | 13 |
| 23 | $\cdot 6\dot{0}5263$ | 15 |
| 21 | $\cdot 5\dot{5}2631$ | 17 |

Thus $\frac{37}{38} = \cdot 9\dot{7}3684$ and the other digits of the period, viz. $210526315789\dot{4}$, are at once completed by the aid of the period at the head of the column: similarly $\frac{35}{38} = \cdot 9\dot{2}1052$ and the rest of the period is completed as before. If the numerator is in the right-hand column, the complementary digits are to be taken: thus $\frac{1}{38} = \cdot 0\dot{2}6315$ and the rest of the period, viz. $789473684210\dot{5}$, is completed as before. When there are several periods, these are all given at the head of the column. The 'tabular series' is similar to the 'tabular series' of 1823 except that it only contains fractions whose numerators and denominators are both not greater than 100 arranged in order of magnitude up to $\frac{1}{2}$. As regards therefore the conversion of vulgar fractions into decimals, the arrangement in the 'first centenary' is, as Mr Goodwyn remarks, much more convenient for entry.

Two years previously (in 1816) Mr Goodwyn had printed for private circulation the 'First centenary.' There is a copy of this earlier edition in the library of the Royal Society: it exactly resembles the 'First centenary' of 1818, but the 'Tabular series' is not added[1]. The introduction to the 'Tabular series' of 1818

[1] There is no title-page in the copy, but the following address appears on the first page.

"The Calculator of about a Chiliad of Tables, from the application of which, in various ways, he has himself derived considerable benefit, has been induced to print the annexed Centenary as a Specimen. Encouraged likewise by Friends—not, perhaps, quite impartial—to give them some publicity, yet still doubtful in himself

commences with the words "Since the 'First Centenary, &c.,' and its Introduction were printed, which was in March, 1816..." and in a paper which appeared in the *Philosophical Magazine* for May, 1816 (vol. xlvii. p. 385) Mr J. Farey speaks of "some curious and elaborate tables of 'Complete decimal Quotients,' calculated by Henry Goodwyn, Esq. of Blackheath, of which he has printed a copious specimen, for private circulation among curious and practical calculators, preparatory to the printing of the whole of these useful Tables, if sufficient encouragement, either public or individual, should appear to warrant such a step."

Mr Farey's[1] paper evidently relates to the 'Tabular series,' and as it seems clear from the contents of the work of 1818 that this appeared for the first time, with its introduction and appendix, as an addition to the 'First Centenary' of 1818, it is to be presumed that Mr Goodwyn showed Mr Farey some portion of the 'tabular series' in manuscript in 1816.

whether they deserve general notice, he adopts this method, which, he trusts, will not be deemed obtrusive or impertinent, of presenting this portion of his labours to a few Individuals. To these Gentlemen, indeed, he has not, in all instances the good fortune of being personally known, but their scientific knowledge and mathematical attainments are highly and justly appreciated; and, it is hoped, that, amongst them, some will have leisure and inclination to honour him with their sentiments on the Specimen, which is thus submitted to their consideration; since he is anxious to confide to their decision, whether the Tables themselves are worthy of publication, or may sink into oblivion with their Author.

"As the above is a private Address, it seems needless for him to add, that the name of any one, who may favour him with his opinion, shall not be divulged without his express consent. Hy. Goodwyn, Blackheath, Kent, March 5th, 1816."

[1] The object of Mr Farey's paper is to draw attention to the following property of vulgar fractions. If all the proper fractions in their lowest terms having both numerator and denominator not greater than a given number $n$, be written down in order of magnitude, then each fraction is equal to a fraction whose numerator and denominator are respectively equal to the sum of the numerators and denominators of the two fractions on each side of it, *e.g.* for $n=7$ the fractions are

$$\tfrac{1}{7},\ \tfrac{1}{6},\ \tfrac{1}{5},\ \tfrac{1}{4},\ \tfrac{2}{7},\ \tfrac{1}{3},\ \tfrac{2}{5},\ \tfrac{1}{2},\ \tfrac{3}{5},\ \tfrac{2}{3},\ \tfrac{5}{7},\ \tfrac{3}{4},\ \tfrac{4}{5},\ \tfrac{5}{6},\ \tfrac{6}{7},\ \text{ and } \tfrac{1}{6}=\frac{1+1}{7+5},\ \tfrac{1}{5}=\frac{1+1}{6+4}\ \text{\&c.}$$

There are two theorems: (i) the difference of any two consecutive fractions is equal to the reciprocal of the product of their denominators; (ii) any three consecutive fractions are connected by the relation mentioned above. Mr Farey observed that the second theorem was true generally for any value of $n$, and published it in the paper in the *Philosophical Magazine* cited above. The first theorem (from which the second is at once deducible) is also true generally, but Mr Farey does not allude to it. An account of Mr Farey's paper appeared in the *Bulletin de la société philomatique de Paris*, t. XII. (1816) p. 112 [by some error the page-numbers 105—112 occur twice]. Cauchy proved both theorems on pp. 133—135 of the same volume, and the proof is reprinted in his *Exercices de Mathématiques*, t. II. (1826) pp. 114—116. Mr Goodwyn mentions both theorems on p. v of the introduction to the 'tabular series' of 1818, but in the introduction to the 'tabular series' of 1823 (p. IV) he only refers to the latter. It thus appears that the second theorem was first published by Mr Farey, and the first by Cauchy. In the *British Association report*, 1873, (p. 33) I have erroneously ascribed both theorems to Cauchy.

[Since this paper was communicated to the Society I have written and sent to the *Philosophical Magazine* a detailed historical account of the two theorems, with demonstrations of them. J. W. L. G., February 20, 1879.]

The Cambridge University Library contains a copy of the 'First centenary, &c.,' 1818, the 'Table of circles,' 1823, and the 'Tabular series,' 1823, the two last being separate. There is bound up with the 'First centenary' the following letter: "September 16th, 1831. Mrs Catherine Goodwyn presents to the Library of the University of Cambridge a complete set of the works of her late father, Henry Goodwyn, Esq., of Blackheath, Kent. Royal Hill, Greenwich." At the end of the 'Tabular series' which is attached to the 'First centenary' of 1818 is a tract of six leaves (one a folding sheet), entitled "Introductory remarks respecting the imperial gallon and diagonal table," followed by a tract of three leaves (one a folding sheet), entitled "Introduction to a synoptical table of English and French lineal measures" (dated 1821, December 13), and three folding sheets relating to weights and measures and simple interest, which were also published by Mr Goodwyn[1].

I have entered thus fully into the description of Mr Goodwyn's works, as they are almost entirely unknown, and the most important of them are anonymous. Reference is made to some of them in De Morgan's article on *Tables* in the Penny and English Cyclopædias, and there is a short account in the British Association report on mathematical tables (1873) pp. 31—33: but I know of but one other place[2] in which any of them are alluded to.

§ 6. It will be at once evident from the description that has been given of Mr Goodwyn's tables how admirably they illustrate the theory of circulating decimals as stated in §§ 2 and 3. The most valuable is the 'table of circles' of 1823 which gives all the periods of the numbers prime to 10 up to 1024. No one who has considered the subject of circulating decimals can fail to find this table one of great interest: it affords copious examples of the rules relating to the periods of circulating decimals, and, besides this, is a really important contribution to arithmetic.

The arrangement adopted by Mr Goodwyn, by which digits and their complements are printed the one under the other is excellent; and the somewhat capricious grouping of the periods which it occasions is no disadvantage.

§ 7. I have had counted, from Mr Goodwyn's 'table of circles,' the number of digits in a period, and the number of periods corresponding to each number; and the results are given in the

[1] The University Library contains duplicates of all these works except the synoptical table and the three folding sheets; but there is no copy of the edition of the 'First centenary' of 1816.

[2] *Mathematical questions from the 'Educational Times,'* vol. ix. (1868), p. 92, where Mr C. W. Merrifield refers to Mr Farey's property.

table annexed to this paper. Adopting the notation of § 2 (i) the table shows the values of $a$ (the number of figures in each period) and $n$ (the number of periods) for every number $q$, prime to 10, from 1 to 1024. The last column in this table gives the value of $na$, found by multiplying $n$ and $a$: this should be equal to $\phi(q)$, and in fact I have in every case calculated $\phi(q)$ independently by means of the formula

$$\phi(q) = q\left(1 - \frac{1}{a}\right)\left(1 - \frac{1}{b}\right)\left(1 - \frac{1}{c}\right)\dots,$$

$a, b, c, \dots$ being the prime factors of $q$, and found that the value of $\phi(q)$ so obtained was the same as the value of $na$.

Extended tables (which are referred to in the next section) have been published of the values of $a$ when $q$ is a prime, and from these by means of the rules in § 2, the values of $a$ and $n$ can be readily found for values much exceeding the limit of the table in this paper. It appears to me however that it is very interesting to have, exhibited in a table, the values of $a$ and $n$, for the numbers from 1 to 1024, and that this interest is greatly enhanced by the fact *that they were obtained by actual counting from the periods themselves*, and were only verified by the rules in § 2. It would in any case be of interest to tabulate the values of $a$ and $n$ for the first thousand numbers—though I do not think it would be worth while to proceed much beyond this limit—but there is a distinct increase of value in such a table when the periods themselves have been actually found in all instances by direct division, and the results obtained from the periods by counting, without the employment of any rules derived from theoretical considerations: in fact I should scarcely have communicated the table to the Society, but for the importance which it seemed to derive from the fact that it was the result of actual observation, and only verified by the theory.

§ 8. Several tables have been published of the numbers of digits in the periods of the reciprocals of primes. Burckhardt at the end of his *Tables des diviseurs pour tous les nombres du premier million* (Paris, 1817) gave such a table for all primes up to 2,543 and for 22 primes exceeding this limit[1]. Desmarest's table on p. 308 of his *Théorie des nombres* (Paris, 1852) includes all primes up to 10,000. Reuschle's *Mathematische Abhandlung enthaltend neue zahlentheoretische Tabellen* (1856)[2] contains a similar table up to 15,000. Mr Shanks has extended the table to 60,000; the

[1] This is the table referred to on pp. 127, 128 of the present volume of *Proceedings*.

[2] For the justification for this date see the *British Association Report* for 1875 (Bristol), p. 311. My copy, like Prof. Cayley's, has no title-page or date.

portion from 1 to 30,000 is printed in the *Proceedings of the Royal Society*, vol. xxii. pp. 200—210 and 384—388, and the remainder is preserved in the archives of the Society (see *Proc. Roy. Soc.* vol. xxiii. p. 260 and vol. xxiv. p. 392). In the formation of these tables, the actual periods were not of course found, but the numbers of digits in the periods were obtained by different processes; the object of these being to determine when the remainder unity first occurs, it being known that this must correspond to a number of quotient digits equal to $p-1$ or to a submultiple of $p-1$, where $p$ denotes the prime. Some account of the methods of finding the number of digits in the period of a prime is given in papers by Mr Shanks and Professor Salmon in the *Messenger of Mathematics*, new series, vol. ii. pp. 41—43, 49—51, 80.

In 1863 Mr G. Suffield of Clare College, and Mr J. R. Lunn of St John's College, Cambridge, published a folding sheet[1] containing the complete period of the prime 7,699 which consists of 7,698 digits, and the process by which the number of digits in the period of this prime was determined beforehand is explained in detail.

Taking this prime as an example, the number of digits in the period must be either 2, 3, 6, 1283, 2566, 3849 or 7698, and the process of finding which of these is the true number consists in determining whether each of the remainders, after 2, 3, 6,... quotient digits have been obtained, is or is not unity. In any case the remainder, after 7698 digits have been obtained, must be unity, and the question is whether a remainder unity presents itself at one of the earlier stages at which it may appear. Through an error of calculation too small a value might be assigned to the number of digits in the period (as *e.g.* 3849 instead of 7698 in this instance); but the converse mistake of obtaining for the number of digits a multiple of the true number (when this number is a submultiple of $p-1$) is much more likely to occur. For supposing the period to contain $a$ digits, where $na=p-1$; then if the remainder after $a$ quotient digits be (wrongly) found not to be unity, but after $ma$ ($m$ being a submultiple of $n$) quotient digits be (rightly) found to be unity, the number of digits in the period would be assigned as $ma$. It is for this reason that some of the processes employed may be unsatisfactory, and that it is not safe to rely upon the complete accuracy of a table of this kind unless confirmed by another table independently calculated. Five years ago I had a comparison made between the tables mentioned at the beginning of this section, and the discrepancies were numerous: a good many of

[1] This sheet, which is dated April 29th, 1863, seems to have been published for private circulation: I was never able to see a copy till Mr W. H. H. Hudson kindly lent me his.

these were decided by the actual performance of the divisions, but the work is still incomplete, though I hope to resume it shortly. It was the special liability to error to which tables of this kind are subject, in connexion with the inaccuracies which I had found to exist, which seemed to make it desirable to publish the table on pp. 204—206; this, being the result of actual counting, is free from possibility of errors such as those alluded to, and may be regarded as a table of observed facts. Extended tables in the higher arithmetic are likely to find their chief use in affording the means of verifying (or even discovering) theoretical laws, and it is important that they should be quite free from error, and particularly from error connected with points which may be essential in the theory.

For the sake of completeness, I may here mention that tables of primes having a given number $n$ of digits in their periods, *i.e.* tables of the resolutions of $10^n - 1$ into factors, and, as far as known, into prime factors, have been given by Loof[1] in t. xvi. (1851) pp. 54—57 of Grunert's *Archiv der Mathematik* and by Mr Shanks in the *Proceedings of the Royal Society*, vol. xxii. pp. 381—384. The former extends from $n = 1$ to $n = 60$, and the latter from $n = 1$ to $n = 100$, but there are of course gaps in both. The tract of Reuschle referred to at the beginning of this section also contains resolutions of $10^n - 1$.

§ 9. Among the posthumous tables at the end of the second volume of Gauss's *Werke* is one entitled *Tafel zur Verwandlung gemeiner Brüche mit Nennern aus dem ersten Tausend in Decimalbrüche,* which occupies pp. 412—434. It consists of two parts. The first contains all the periods of the primes and powers of primes from 3 to 463, and the second the period of the reciprocal of every prime and power of a prime from 467 to 997. Thus up to 463 Gauss's table is the same as Mr Goodwyn's except that it includes only primes and powers of primes, while the latter includes all numbers prime to 10; above 463 there is the additional restriction that only one period is given. In Mr Goodwyn's table the periods are arranged in order of magnitude, *i.e.* so that each period as it stands is equal to the least fraction to which it belongs, and that the periods regarded as decimal fractions are in order of magnitude[2]; but in Gauss's table the periods of $q$ which are marked (1), (2), (3), ... (0) correspond to the periods of

$$\frac{10r}{q}, \frac{10r^2}{q}, \frac{10r^3}{q}, \dots \frac{10}{q},$$

[1] This table was reprinted in the *Nouvelles Annales de Mathématiques,* t. xiv. (1855), pp. 115—117.

[2] According to this rule the last period of 73 (quoted in § 2 (v), p. 187) should have been printed $\cdot\dot{2}465\ 753\dot{4}$. The periods are generally arranged in order of magnitude, and this exception was probably accidental.

where $r$ is the least of those primitive roots of $q$ for which as base the index of 10 is least, the values of $r$, corresponding to the different values of $q$, being given at the end of the table[1]. It is stated by the editor, Dr Schering, (p. 497) that the manuscript book from which the table was taken ends with the words: *Explicitus October* 11, 1795. This table is referred to in Art. 316 of the *Disquisitiones arithmeticæ* which begins "Secundum hæc principia pro omnibus denominatoribus formæ $p^{\mu}$ infra 1000 tabulam periodorum necessariarum construximus, quam integram sive etiam ulterius continuatam occasione data publici juris faciemus." Tabula III. at the end of the *Disquisitiones* is a similar table extending only as far as 100: but there are differences in points of detail. It will be seen that the table in vol. ii. of Gauss's *Werke* is not so complete, or for some purposes so conveniently arranged, as Mr Goodwyn's 'table of circles.' I have had the two tables compared, and the discrepancies decided by division. As was to be expected, considering the circumstances of the publication of Gauss's table, the values in Goodwyn were found to be the more correct: but the results of this comparison are not at present in a fit state for publication.

§ 10. With regard to the arrangement of a table for the conversion of vulgar fractions into decimals, the most elegant theoretical method would be, I think, to give as it were the full divisions in the form in which Mr Goodwyn actually performed them. Thus the actual divisions for the number 39 are

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 39) 1 | ($\cdot\dot{0}$ | 39) 38 | ($\cdot\dot{9}$ | 39) 2 | ($\cdot\dot{0}$ | 39) 37 | ($\cdot\dot{9}$ |
| 10 | 2 | 29 | 7 | 20 | 5 | 19 | 4 |
| 22 | 5 | 17 | 4 | 5 | 1 | 34 | 8 |
| 25 | 6 | 14 | 3 | 11 | 2 | 28 | 7 |
| 16 | 4 | 23 | 5 | 32 | 8 | 7 | 1 |
| 4 | $\dot{1}$ | 35 | $\dot{8}$ | 8 | $\dot{2}$ | 31 | $\dot{7}$ |

and this might be printed:

| 39 | | | | | | | |
|---|---|---|---|---|---|---|---|
| 1 | 0 | 38 | 9 | 2 | 0 | 37 | 9 |
| 10 | 2 | 29 | 7 | 20 | 5 | 19 | 4 |
| 22 | 5 | 17 | 4 | 5 | 1 | 34 | 8 |
| 25 | 6 | 14 | 3 | 11 | 2 | 28 | 7 |
| 16 | 4 | 23 | 5 | 32 | 8 | 7 | 1 |
| 4 | 1 | 35 | 8 | 8 | 2 | 31 | 7 |

[1] Gauss's table is explained in some detail by Professor Cayley, *British Association Report*, 1875 (Bristol), pp. 317—318.

Thus for example $\frac{16}{39} = \cdot\dot{4}1025\dot{6}$, $\frac{14}{39} = \cdot\dot{3}5897\dot{4}$, $\frac{5}{39} = \cdot\dot{1}2820\dot{5}$, &c. With this arrangement, it would be necessary to search among the columns of arguments for the particular numerator required; but each period would only be printed once, and all the numerators having the same period would be placed together.

§ 11. The property of the number 487 referred to in § 2 (x), viz. that the periods of $\frac{1}{487}$ and $(\frac{1}{487})^2$ both contain 486 digits, was, I believe, discovered by Desmarest, who seems to have divided the period of each number up to 1000 by the number and found that in no other case except that of 3 and 487 was the period a multiple of the number. For he enunciates the rule in § 2 (x) as follows[1]: "1° Si on transforme en fractions de l'ordre décimal une fraction $\frac{A}{P^2}$, le nombre $P$ premier absolu étant inférieur à 1000; 2° si on désigne par $p$ le nombre des chiffres de la période donnée par la fraction $\frac{A}{P}$; 3° si on excepte les nombres premiers 3, 487; le nombre de chiffres de la période inconnue, c'est-à-dire de la période donnée par la fraction $\frac{A}{P^2}$, est le nombre entier $p \,.\, P$." These words and others distinctly imply that 3 and 487 are the only exceptions to the general rule up to 1000. In order to establish this and to find that 487 was an exception, Desmarest must have performed the divisions (or employed some equivalent process); but it seems strange that if he had actually performed this heavy work he should not have stated the fact explicitly. On the other hand, I have been able to find no allusion to the property of the number 487 prior to the date of Desmarest's work; and it is scarcely to be supposed that Desmarest would adopt so important a statement as that quoted above without giving his authority.

I have verified Desmarest's statement with regard to 487, and I give the value of $\frac{1}{487}$ and $(\frac{1}{487})^2, = \frac{1}{237169}$, as obtained by actual division.

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| $\frac{1}{487} =$ | $\cdot\dot{0}0205$ | 33880 | 90349 | 07597 | 53593 | 42915 | 81108 | 82956 | 87885[2] | 01026 |
| | 69404 | 51745 | 37987 | 67967 | 14579 | 05544 | 14784 | 39425 | 05133 | 47022 |
| | 58726 | 89938 | 39835 | 72895 | 27720 | 73921 | 97125 | 25667 | 35112 | 93634 |
| | 49691 | 99178 | 64476 | 38603 | 69609 | 85626 | 28336 | 75564 | 68172 | 48459 |
| | 95893 | 22381 | 93018 | 48049 | 28131 | 41683 | 77823 | 40862 | 42299 | 79466 |
| | 11909 | 65092 | 40246 | 40657 | 08418 | 89117 | 04312 | 11498 | 97330 | 59548 |
| | 25462 | 01232 | 03285 | 42094 | 45585 | 21560 | 57494 | 86652 | 97741 | 27310 |
| | 06160 | 16427 | 10472 | 27926 | 07802 | 87474 | 33264 | 88706 | 36550 | 30800 |
| | 82135 | 52361 | 39630 | 39014 | 37371 | 66324 | 43531 | 82751 | 54004 | 10677 |
| | 61806 | 98151 | 95071 | 86858 | 31622 | 17659 | 13757 | $\dot{7}$ | | |

[1] *Théorie des nombres*, pp. 294—295.

[2] It may be noticed that the remainder following this quotient digit is 5, so that the succeeding figures 01026... to the end of the period are written down at once by halving the part of the period already obtained, beginning with the second digit.

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| $\frac{1}{487^2}=\dot{\cdot}00000$ | 42164 | 02649 | 58742 | 50007 | 37870 | 46367 | 77993 | 75129 | 12733 |
| | 11436 | 14890 | 64759 | 72829 | 50132 | 60586 | 33295 | 24516 | 27320 | 60260 |
| | 82666 | 79034 | 78110 | 54564 | 46668 | 83108 | 66934 | 54878 | 16704 | 54401 |
| | 71354 | 60367 | 92329 | 52029 | 98705 | 56438 | 65766 | 60524 | 77347 | 37676 |
| | 50915 | 59183 | 53579 | 09338 | 91022 | 85711 | 87634 | 13430 | 92899 | 99957 |
| | 83597 | 35041 | 25749 | 99262 | 12953 | 63222 | 00624 | 87087 | 26688 | 56385 |
| | 10935 | 24027 | 17049 | 86739 | 41366 | 70475 | 48372 | 67939 | 73917 | 33320 |
| | 96521 | 88945 | 43553 | 33116 | 89133 | 06545 | 12183 | 29545 | 59828 | 64539 |
| | 63207 | 67047 | 97001 | 29443 | 56134 | 23339 | 47522 | 65262 | 32349 | 08440 |
| | 81646 | 42090 | 66108 | 97714 | 28812 | 36586 | 56907 | $\dot{1}$ | | |

I have also verified that the latter period is not divisible by 487, so that the period of $(\frac{1}{487})^2$ contains $486 \times 487$ digits.

Since $$10^{486}-1=(10^{243}+1)(10^{243}-1)$$
and $$10^{243}+1=(10^{81}+1)(10^{162}-10^{81}+1)$$
it follows that $10^{162}-10^{81}+1$ is divisible by 487 and by $(487)^2$. The quotients thus obtained are given by Mr Shanks in vol. xxv., p. 553, of the *Proceedings of the Royal Society* (1877). The prime 69499 has a period of 486 digits, and therefore $\frac{1}{487}(10^{162}-10^{81}+1)$ and $(\frac{1}{487})^2(10^{162}-10^{81}+1)$ are both divisible by 69499: this latter quotient is also given by Mr Shanks.

The fact that 487 is such that $10^{487} \equiv 1 \pmod{487^2}$ is interesting for the following reason. In t. III. (1828), p. 212, of *Crelle's Journal*, Abel proposed the question, "Kann $\alpha^{\mu-1}-1$, wenn $\mu$ eine Primzahl und $\alpha$ eine ganze Zahl und kleiner als $\mu$ und grösser als 1 ist, durch $\mu^2$ theilbar sein?" On pp. 301—302, Jacobi answered this question by showing that $3^{10} \equiv 1 \pmod{11^2}$, so that $9^{10} \equiv 1 \pmod{11^2}$, and also that $14^{28} \equiv 1 \pmod{29^2}$ and $18^{36} \equiv 1 \pmod{37^2}$. The case of 487 is a solution of Abel's congruence $\alpha^{\mu-1} \equiv 1 \pmod{\mu^2}$, $\mu > \alpha$, when $\alpha = 10$, and is the only known solution. There seems no reason to suppose that there are not other solutions, and that the congruences $10^{\mu-1} \equiv 1 \pmod{\mu^3}$, &c., may not have solutions. The next solution above 487 of the congruence $10^{\mu-1} \equiv 1 \pmod{\mu^2}$ may be a very high number, as is evident by merely considering the diminution of the chance of a number dividing exactly its own period—the latter being regarded merely as a number taken at random—as the number increases and consequently the number of possible remainders increases; the period being of course divisible by the number only in the case when the last remainder is zero. A computer finding the rule in § 2 (x) to be true for all the numbers except 3, to which he applied it, might believe it to be universally true, with this sole exception, and actually use it in forming a table such as that at the end of this paper. This affords a good instance of the necessity, in the case of a table in Theory of Numbers, of fully explaining the mode of construction; as nothing can be more unsafe than the use of empirical rules in the Theory of Numbers

where the induction has to be made from a comparatively small number of instances and those all near the beginning of the series of numerals.

§ 12. The rules given in § 2 are not to be regarded as in any sense a complete account of the properties of circulating decimals; they merely contain a brief statement of the principal results which are illustrated by Mr Goodwyn's tables. Among others of less importance may be noticed the following. If $q$ be a prime ending with 1, viz. $= 10m + 1$, then each of the digits 0, 1, 2, ... 9 occurs $m$ times in the $10m$ digits which form the periods of $q$. For example, if $q = 41$ the periods are

$$\cdot\dot{0}243\dot{9} \qquad \cdot\dot{0}487\dot{8} \qquad \cdot\dot{0}731\dot{7} \qquad \cdot\dot{1}463\dot{4}$$
$$\cdot\dot{9}756\dot{0} \qquad \cdot\dot{9}512\dot{1} \qquad \cdot\dot{9}268\dot{2} \qquad \cdot\dot{8}536\dot{5}$$

and there are four 0's, four 1's, four 2's, &c. If $q$ has only one period[1] ($q = 61$ is the lowest number of the given form for which this is true) this period contains an equal number of 0's, 1's, 2's, ... 9's.

It may be useful to refer briefly to the principal memoirs and writings relating to the theory of circulating decimals. The most considerable memoir that I have seen is one by John Bernoulli, *Sur les fractions décimales périodiques,* which, with an addition, occupies pp. 273—317 of the *Berlin Memoirs* for 1771. Besides his own investigations Bernoulli gives a full account of the contents of Chapter XII., Book I, of Euler's Algebra, of Chapter LXXXIX. of Wallis's Algebra, and of Robertson's paper *Of the theory of circulating fractions* (*Phil. Trans.* 1768), all of which relate to the subject of circulating decimals. In the addition to the memoir he also gives an account of papers by Lambert, in the third volume of the *Acta Helvetica*, printed in 1758, and in the *Nova Acta Eruditorum* for March 1769. The memoir, with the addition, thus contains a full explanation of what was then known on the subject: but its interest is now chiefly historical. Arts. 308—318 of Gauss's *Disquisitiones arithmeticæ* (*sectio sexta*) relate, as is well known, to circulating decimals. In vol. I. (1842), pp. 457—470, of the *Nouvelles Annales*, M. Catalan stated and proved several of the more elementary properties of circulating decimals. In Desmarest's *Théorie des nombres,* already referred to, Art. 143 (pp. 289—296) is entitled "De la transformation d'une fraction ordinaire $\frac{A}{H}$, dite ancienne, en fractions de l'ordre $\epsilon$, et plus particulièrement en fractions de l'ordre décimal," and relates to the number of digits in the periods. Mr W. H. H. Hudson's paper *On primes and proper*

[1] *Messenger of Mathematics*, First series, vol. II. p. 4.

*primes,* which was published anonymously in the first series of the *Messenger of Mathematics,* vol. II. pp. 1—6, is a short but valuable contribution to the subject. When engaged during the early part of the present year in forming as complete a list as possible of the rules relating to circulating decimals, I found this paper more useful than any other that I met with. On p. 39 of the same volume of the *Messenger* the full decimal value of $\frac{1}{1861}$ (which has a period of 1860 digits) is given. This was intended to illustrate Mr Hudson's paper, and it is pointed out that the complementary portion begins at the 931st digit, the double at the 1202nd, the half at the 660th, &c. The number of the *Messenger* containing Mr Hudson's paper, and the period of 1861, was published in March, 1863, and as the folding sheet of Messrs Suffield and Lunn containing the period of 7,699 (see § 8) is dated April 29, 1863, the latter was probably a consequence of Mr Hudson's writings. Mr Shanks subsequently calculated the period of the prime 17,389, which contains 17,388 digits, but it has not been printed[1]. Mr Suffield published in 1863[2] a tract, *Synthetic division in arithmetic, with some introductory remarks on the period of circulating decimals* (pp. iv. + 19), and also, on a separate leaf, *A specimen of 'Synthetic Division'* (without date, but probably issued immediately after the tract). The 'introductory remarks on the period of circulating decimals,' which occupy three pages, contain the more important rules on the subject, and the whole method of synthetic division is closely connected with the theory of circulating decimals, so that the work is an important one. As the book is scarce it may be convenient to mention that *The rationale of circulating numbers* by Henry Clarke (London, 1777 and 1794), merely contains the ordinary arithmetical processes for the addition of circulating decimals, conversions of vulgar fractions into decimals, &c., and does not relate to the theoretical principles. I do not know of any account of the properties of circulating decimals which at all approaches completeness; and the list of rules in § 2, although purposely restricted, is more complete than any account that I have met with. On the other hand, however, it would be very difficult to discover any property that had not been previously published in some form or another.

[1] See *Proc. Roy. Soc.* vol. xv. p. 429 (1867).
[2] The preface is dated January, 1863.

TABLE OF THE PERIODS OF FRACTIONS CORRESPONDING TO DENOMINATORS, PRIME TO 10, FROM 1 TO 1024.

*The table is explained in* § 6, pp. 195—196.

| $q$ | $a$ | $n$ | $\phi(q)$ | $q$ | $a$ | $n$ | $\phi(q)$ | $q$ | $a$ | $n$ | $\phi(q)$ |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 3 | 1 | 2 | 2 | 121 | 22 | 5 | 110 | 239 | 7 | 34 | 238 |
| 7 | 6 | 1 | 6 | 123 | 5 | 16 | 80 | 241 | 30 | 8 | 240 |
| 9 | 1 | 6 | 6 | 127 | 42 | 3 | 126 | 243 | 27 | 6 | 162 |
| 11 | 2 | 5 | 10 | 129 | 21 | 4 | 84 | 247 | 18 | 12 | 216 |
| 13 | 6 | 2 | 12 | 131 | 130 | 1 | 130 | 249 | 41 | 4 | 164 |
| 17 | 16 | 1 | 16 | 133 | 18 | 6 | 108 | 251 | 50 | 5 | 250 |
| 19 | 18 | 1 | 18 | 137 | 8 | 17 | 136 | 253 | 22 | 10 | 220 |
| 21 | 6 | 2 | 12 | 139 | 46 | 3 | 138 | 257 | 256 | 1 | 256 |
| 23 | 22 | 1 | 22 | 141 | 46 | 2 | 92 | 259 | 6 | 36 | 216 |
| 27 | 3 | 6 | 18 | 143 | 6 | 20 | 120 | 261 | 28 | 6 | 168 |
| 29 | 28 | 1 | 28 | 147 | 42 | 2 | 84 | 263 | 262 | 1 | 262 |
| 31 | 15 | 2 | 30 | 149 | 148 | 1 | 148 | 267 | 44 | 4 | 176 |
| 33 | 2 | 10 | 20 | 151 | 75 | 2 | 150 | 269 | 268 | 1 | 268 |
| 37 | 3 | 12 | 36 | 153 | 16 | 6 | 96 | 271 | 5 | 54 | 270 |
| 39 | 6 | 4 | 24 | 157 | 78 | 2 | 156 | 273 | 6 | 24 | 144 |
| 41 | 5 | 8 | 40 | 159 | 13 | 8 | 104 | 277 | 69 | 4 | 276 |
| 43 | 21 | 2 | 42 | 161 | 66 | 2 | 132 | 279 | 15 | 12 | 180 |
| 47 | 46 | 1 | 46 | 163 | 81 | 2 | 162 | 281 | 28 | 10 | 280 |
| 49 | 42 | 1 | 42 | 167 | 166 | 1 | 166 | 283 | 141 | 2 | 282 |
| 51 | 16 | 2 | 32 | 169 | 78 | 2 | 156 | 287 | 30 | 8 | 240 |
| 53 | 13 | 4 | 52 | 171 | 18 | 6 | 108 | 289 | 272 | 1 | 272 |
| 57 | 18 | 2 | 36 | 173 | 43 | 4 | 172 | 291 | 96 | 2 | 192 |
| 59 | 58 | 1 | 58 | 177 | 58 | 2 | 116 | 293 | 146 | 2 | 292 |
| 61 | 60 | 1 | 60 | 179 | 178 | 1 | 178 | 297 | 6 | 30 | 180 |
| 63 | 6 | 6 | 36 | 181 | 180 | 1 | 180 | 299 | 66 | 4 | 264 |
| 67 | 33 | 2 | 66 | 183 | 60 | 2 | 120 | 301 | 42 | 6 | 252 |
| 69 | 22 | 2 | 44 | 187 | 16 | 10 | 160 | 303 | 4 | 50 | 200 |
| 71 | 35 | 2 | 70 | 189 | 6 | 18 | 108 | 307 | 153 | 2 | 306 |
| 73 | 8 | 9 | 72 | 191 | 95 | 2 | 190 | 309 | 34 | 6 | 204 |
| 77 | 6 | 10 | 60 | 193 | 192 | 1 | 192 | 311 | 155 | 2 | 310 |
| 79 | 13 | 6 | 78 | 197 | 98 | 2 | 196 | 313 | 312 | 1 | 312 |
| 81 | 9 | 6 | 54 | 199 | 99 | 2 | 198 | 317 | 79 | 4 | 316 |
| 83 | 41 | 2 | 82 | 201 | 33 | 4 | 132 | 319 | 28 | 10 | 280 |
| 87 | 28 | 2 | 56 | 203 | 84 | 2 | 168 | 321 | 53 | 4 | 212 |
| 89 | 44 | 2 | 88 | 207 | 22 | 6 | 132 | 323 | 144 | 2 | 288 |
| 91 | 6 | 12 | 72 | 209 | 18 | 10 | 180 | 327 | 108 | 2 | 216 |
| 93 | 15 | 4 | 60 | 211 | 30 | 7 | 210 | 329 | 138 | 2 | 276 |
| 97 | 96 | 1 | 96 | 213 | 35 | 4 | 140 | 331 | 110 | 3 | 330 |
| 99 | 2 | 30 | 60 | 217 | 30 | 6 | 180 | 333 | 3 | 72 | 216 |
| 101 | 4 | 25 | 100 | 219 | 8 | 18 | 144 | 337 | 336 | 1 | 336 |
| 103 | 34 | 3 | 102 | 221 | 48 | 4 | 192 | 339 | 112 | 2 | 224 |
| 107 | 53 | 2 | 106 | 223 | 222 | 1 | 222 | 341 | 30 | 10 | 300 |
| 109 | 108 | 1 | 108 | 227 | 113 | 2 | 226 | 343 | 294 | 1 | 294 |
| 111 | 3 | 24 | 72 | 229 | 228 | 1 | 228 | 347 | 173 | 2 | 346 |
| 113 | 112 | 1 | 112 | 231 | 6 | 20 | 120 | 349 | 116 | 3 | 348 |
| 117 | 6 | 12 | 72 | 233 | 232 | 1 | 232 | 351 | 6 | 36 | 216 |
| 119 | 48 | 2 | 96 | 237 | 13 | 12 | 156 | 353 | 32 | 11 | 352 |

| $q$ | $a$ | $n$ | $\phi(q)$ | $q$ | $a$ | $n$ | $\phi(q)$ | $q$ | $a$ | $n$ | $\phi(q)$ |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 357 | 48 | 4 | 192 | 489 | 81 | 4 | 324 | 621 | 66 | 6 | 396 |
| 359 | 179 | 2 | 358 | 491 | 490 | 1 | 490 | 623 | 132 | 4 | 528 |
| 361 | 342 | 1 | 342 | 493 | 112 | 4 | 448 | 627 | 18 | 20 | 360 |
| 363 | 22 | 10 | 220 | 497 | 210 | 2 | 420 | 629 | 48 | 12 | 576 |
| 367 | 366 | 1 | 366 | 499 | 498 | 1 | 498 | 631 | 315 | 2 | 630 |
| 369 | 5 | 48 | 240 | 501 | 166 | 2 | 332 | 633 | 30 | 14 | 420 |
| 371 | 78 | 4 | 312 | 503 | 502 | 1 | 502 | 637 | 42 | 12 | 504 |
| 373 | 186 | 2 | 372 | 507 | 78 | 4 | 312 | 639 | 35 | 12 | 420 |
| 377 | 84 | 4 | 336 | 509 | 508 | 1 | 508 | 641 | 32 | 20 | 640 |
| 379 | 378 | 1 | 378 | 511 | 24 | 18 | 432 | 643 | 107 | 6 | 642 |
| 381 | 42 | 6 | 252 | 513 | 18 | 18 | 324 | 647 | 646 | 1 | 646 |
| 383 | 382 | 1 | 382 | 517 | 46 | 10 | 460 | 649 | 58 | 10 | 580 |
| 387 | 21 | 12 | 252 | 519 | 43 | 8 | 344 | 651 | 30 | 12 | 360 |
| 389 | 388 | 1 | 388 | 521 | 52 | 10 | 520 | 653 | 326 | 2 | 652 |
| 391 | 176 | 2 | 352 | 523 | 261 | 2 | 522 | 657 | 8 | 54 | 432 |
| 393 | 130 | 2 | 260 | 527 | 240 | 2 | 480 | 659 | 658 | 1 | 658 |
| 397 | 99 | 4 | 396 | 529 | 506 | 1 | 506 | 661 | 220 | 3 | 660 |
| 399 | 18 | 12 | 216 | 531 | 58 | 6 | 348 | 663 | 48 | 8 | 384 |
| 401 | 200 | 2 | 400 | 533 | 30 | 16 | 480 | 667 | 308 | 2 | 616 |
| 403 | 30 | 12 | 360 | 537 | 178 | 2 | 356 | 669 | 222 | 2 | 444 |
| 407 | 6 | 60 | 360 | 539 | 42 | 10 | 420 | 671 | 60 | 10 | 600 |
| 409 | 204 | 2 | 408 | 541 | 540 | 1 | 540 | 673 | 224 | 3 | 672 |
| 411 | 8 | 34 | 272 | 543 | 180 | 2 | 360 | 677 | 338 | 2 | 676 |
| 413 | 174 | 2 | 348 | 547 | 91 | 6 | 546 | 679 | 96 | 6 | 576 |
| 417 | 46 | 6 | 276 | 549 | 60 | 6 | 360 | 681 | 113 | 4 | 452 |
| 419 | 418 | 1 | 418 | 551 | 252 | 2 | 504 | 683 | 341 | 2 | 682 |
| 421 | 140 | 3 | 420 | 553 | 78 | 6 | 468 | 687 | 228 | 2 | 456 |
| 423 | 46 | 6 | 276 | 557 | 278 | 2 | 556 | 689 | 78 | 8 | 624 |
| 427 | 60 | 6 | 360 | 559 | 42 | 12 | 504 | 691 | 230 | 3 | 690 |
| 429 | 6 | 40 | 240 | 561 | 16 | 20 | 320 | 693 | 6 | 60 | 360 |
| 431 | 215 | 2 | 430 | 563 | 281 | 2 | 562 | 697 | 80 | 8 | 640 |
| 433 | 432 | 1 | 432 | 567 | 18 | 18 | 324 | 699 | 232 | 2 | 464 |
| 437 | 198 | 2 | 396 | 569 | 284 | 2 | 568 | 701 | 700 | 1 | 700 |
| 439 | 219 | 2 | 438 | 571 | 570 | 1 | 570 | 703 | 18 | 36 | 648 |
| 441 | 42 | 6 | 252 | 573 | 95 | 4 | 380 | 707 | 12 | 50 | 600 |
| 443 | 221 | 2 | 442 | 577 | 576 | 1 | 576 | 709 | 708 | 1 | 708 |
| 447 | 148 | 2 | 296 | 579 | 192 | 2 | 384 | 711 | 13 | 36 | 468 |
| 449 | 32 | 14 | 448 | 581 | 246 | 2 | 492 | 713 | 330 | 2 | 660 |
| 451 | 10 | 40 | 400 | 583 | 26 | 20 | 520 | 717 | 7 | 68 | 476 |
| 453 | 75 | 4 | 300 | 587 | 293 | 2 | 586 | 719 | 359 | 2 | 718 |
| 457 | 152 | 3 | 456 | 589 | 90 | 6 | 540 | 721 | 102 | 6 | 612 |
| 459 | 48 | 6 | 288 | 591 | 98 | 4 | 392 | 723 | 30 | 16 | 480 |
| 461 | 460 | 1 | 460 | 593 | 592 | 1 | 592 | 727 | 726 | 1 | 726 |
| 463 | 154 | 3 | 462 | 597 | 99 | 4 | 396 | 729 | 81 | 6 | 486 |
| 467 | 233 | 2 | 466 | 599 | 299 | 2 | 598 | 731 | 336 | 2 | 672 |
| 469 | 66 | 6 | 396 | 601 | 300 | 2 | 600 | 733 | 61 | 12 | 732 |
| 471 | 78 | 4 | 312 | 603 | 33 | 12 | 396 | 737 | 66 | 10 | 660 |
| 473 | 42 | 10 | 420 | 607 | 202 | 3 | 606 | 739 | 246 | 3 | 738 |
| 477 | 13 | 24 | 312 | 609 | 84 | 4 | 336 | 741 | 18 | 24 | 432 |
| 479 | 239 | 2 | 478 | 611 | 138 | 4 | 552 | 743 | 742 | 1 | 742 |
| 481 | 6 | 72 | 432 | 613 | 51 | 12 | 612 | 747 | 41 | 12 | 492 |
| 483 | 66 | 4 | 264 | 617 | 88 | 7 | 616 | 749 | 318 | 2 | 636 |
| 487 | 486 | 1 | 486 | 619 | 618 | 1 | 618 | 751 | 125 | 6 | 750 |

| $q$ | $a$ | $n$ | $\phi(q)$ | $q$ | $a$ | $n$ | $\phi(q)$ | $q$ | $a$ | $n$ | $\phi(q)$ |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 753 | 50 | 10 | 500 | 847 | 66 | 10 | 660 | 939 | 312 | 2 | 624 |
| 757 | 27 | 28 | 756 | 849 | 141 | 4 | 564 | 941 | 940 | 1 | 940 |
| 759 | 22 | 20 | 440 | 851 | 66 | 12 | 792 | 943 | 110 | 8 | 880 |
| 761 | 380 | 2 | 760 | 853 | 213 | 4 | 852 | 947 | 473 | 2 | 946 |
| 763 | 108 | 6 | 648 | 857 | 856 | 1 | 856 | 949 | 24 | 36 | 864 |
| 767 | 174 | 4 | 696 | 859 | 26 | 33 | 858 | 951 | 79 | 8 | 632 |
| 769 | 192 | 4 | 768 | 861 | 30 | 16 | 480 | 953 | 952 | 1 | 952 |
| 771 | 256 | 2 | 512 | 863 | 862 | 1 | 862 | 957 | 28 | 20 | 560 |
| 773 | 193 | 4 | 772 | 867 | 272 | 2 | 544 | 959 | 24 | 34 | 816 |
| 777 | 6 | 72 | 432 | 869 | 26 | 30 | 780 | 961 | 465 | 2 | 930 |
| 779 | 90 | 8 | 720 | 871 | 66 | 12 | 792 | 963 | 53 | 12 | 636 |
| 781 | 70 | 10 | 700 | 873 | 96 | 6 | 576 | 967 | 322 | 3 | 966 |
| 783 | 84 | 6 | 504 | 877 | 438 | 2 | 876 | 969 | 144 | 4 | 576 |
| 787 | 393 | 2 | 786 | 879 | 146 | 4 | 584 | 971 | 970 | 1 | 970 |
| 789 | 262 | 2 | 524 | 881 | 440 | 2 | 880 | 973 | 138 | 6 | 828 |
| 791 | 336 | 2 | 672 | 883 | 441 | 2 | 882 | 977 | 976 | 1 | 976 |
| 793 | 60 | 12 | 720 | 887 | 886 | 1 | 886 | 979 | 44 | 20 | 880 |
| 797 | 199 | 4 | 796 | 889 | 42 | 18 | 756 | 981 | 108 | 6 | 648 |
| 799 | 368 | 2 | 736 | 891 | 18 | 30 | 540 | 983 | 982 | 1 | 982 |
| 801 | 44 | 12 | 528 | 893 | 414 | 2 | 828 | 987 | 138 | 4 | 552 |
| 803 | 8 | 90 | 720 | 897 | 66 | 8 | 528 | 989 | 462 | 2 | 924 |
| 807 | 268 | 2 | 536 | 899 | 420 | 2 | 840 | 991 | 495 | 2 | 990 |
| 809 | 202 | 4 | 808 | 901 | 208 | 4 | 832 | 993 | 110 | 6 | 660 |
| 811 | 810 | 1 | 810 | 903 | 42 | 12 | 504 | 997 | 166 | 6 | 996 |
| 813 | 5 | 108 | 540 | 907 | 151 | 6 | 906 | 999 | 3 | 216 | 648 |
| 817 | 126 | 6 | 756 | 909 | 4 | 150 | 600 | 1001 | 6 | 120 | 720 |
| 819 | 6 | 72 | 432 | 911 | 455 | 2 | 910 | 1003 | 464 | 2 | 928 |
| 821 | 820 | 1 | 820 | 913 | 82 | 10 | 820 | 1007 | 234 | 4 | 936 |
| 823 | 822 | 1 | 822 | 917 | 390 | 2 | 780 | 1009 | 252 | 4 | 1008 |
| 827 | 413 | 2 | 826 | 919 | 459 | 2 | 918 | 1011 | 336 | 2 | 672 |
| 829 | 276 | 3 | 828 | 921 | 153 | 4 | 612 | 1013 | 253 | 4 | 1012 |
| 831 | 69 | 8 | 552 | 923 | 210 | 4 | 840 | 1017 | 112 | 6 | 672 |
| 833 | 336 | 2 | 672 | 927 | 34 | 18 | 612 | 1019 | 1018 | 1 | 1018 |
| 837 | 15 | 36 | 540 | 929 | 464 | 2 | 928 | 1021 | 1020 | 1 | 1020 |
| 839 | 419 | 2 | 838 | 931 | 126 | 6 | 756 | 1023 | 30 | 20 | 600 |
| 841 | 812 | 1 | 812 | 933 | 155 | 4 | 620 | | | | |
| 843 | 28 | 20 | 560 | 937 | 936 | 1 | 936 | | | | |

[$a$ = number of digits in a period,
$n$ = number of periods,
$\phi(q)$, $= na$, = number of numbers less than $q$ and prime to it].

*November* 4, 1878.

DR J. B. PEARSON, TREASURER, IN THE CHAIR.

Mr R. C. ROWE, B.A., Trinity College, and Mr W. J. SELL, B.A., Christ's College, were ballotted for and duly elected Fellows of the Society.

The following communications were made to the Society:—

(1) Professor DEWAR, F.R.S. *The physical constants of hydrogenium*, Part II.

This paper is a continuation of an investigation into the Physical Constants of Hydrogenium. The first part appeared in the *Transactions of the Royal Society of Edinburgh*, Vol. XXVII., and had reference to the Specific Gravity, Specific Heat, and Coefficient of Expansion of the occluded hydrogen. These observations led to the conclusion that the specific gravity was independent of the amount of condensed gas, and had a mean value of 0·62. This result has been confirmed by the subsequent experiments of Troost and Hautefeuille, and what is very remarkable, they deduce an identical value for the density of hydrogen from observations on the hydrides of potassium and sodium. The specific heat, relatively to palladium, of the condensed hydrogen, appeared to vary inversely as the charge, but taken relatively to successive charges was nearly constant, and had the value 3·4, which is identical with that of gaseous hydrogen at constant pressure. The coefficient of the cubical expansion of the alloy is about twice that of palladium, and that of the hydrogen in its compressed state not more than three times that of mercury. This communication deals with the Thermo-electric Relations and Conductivity of Hydrogenium. It is shown that the electromotive force of a junction of hydrogenium palladium is at ordinary temperature nearly equal to that of an iron copper junction, and that it increases with the temperature according to the general parabolic law; the rate of the increase being however greater than iron copper and subject to a regular variation on account of successive heatings. The formation of thermo-electric piles, and of neutral points in a uniform wire of this substance, along with the continuous formation of thermo-electric currents through the application of a hydrogen flame were explained and shown. Experiments on the electric resistance shew that it increases directly as the amount of condensed gas.

(2) Professors LIVEING and DEWAR. *Studies in spectrum analysis.*

The authors describe the reversal of characteristic lines of rubidium and cœsium when the chlorides are heated with sodium in glass tubes in an atmosphere of hydrogen or nitrogen, and a bright light is viewed through the vapours. They remark that the violet lines of rubidium, and the most refrangible of the cœsium lines are first seen, and broaden out the most when the temperature rises, contrary to what might have been expected from the analogy of other cases. The absorption lines observed coincided with the bright lines of the metals heated in a flame, not with the lines which they give in a dense electric spark; but the authors obtained spectra similar to the flame spectra by passing sparks from an induction coil, without a Leyden jar, between beads of fused chlorides of those metals, although simpler spectra were produced by the more abrupt discharges produced by interposing a Leyden jar. The authors further described absorption spectra produced by magnesium vapour when mixed with hydrogen, potassium, and sodium respectively. That produced by magnesium and hydrogen consisted of a line a little less refrangible than the *b* group, and a band rather more refrangible than the *b* group, fading away towards the blue. The constant appearance of these absorptions when the vapour of magnesium in hydrogen was observed in a hot iron tube, led to the endeavour to obtain the corresponding luminous spectrum. This they succeeded in doing by taking sparks from an induction coil, without a Leyden jar, between magnesium wires in a tube full of hydrogen. It appears that the compound to which this spectrum is due is formed only within a certain range of temperature, and is dissociated at higher temperatures—for the spectrum is scarcely seen at all when a large Leyden jar is used, which may be supposed to have the effect of shortening the time of discharge and increasing the temperature. Further, this compound does not seem to be formed when the pressure of the hydrogen is much reduced. In the case of sodium and magnesium they observed an absorption line in the green not observed in either vapour separately; and when potassium and magnesium were used, a characteristic pair of lines in the red always appeared, and sometimes another line in the blue. The authors have not yet seen these as bright lines. In the course of observations on the spectra of sundry rarified gases the authors have been led to conclude that electric sparks take a selective course in a mixture of gases, and that the differences in the spectra observed in different parts of the same tube are probably due to the existence of more than one gas in the tube. Tubes of nitrogen which did not shew the lines of hydrogen at all when sparks from

an induction coil without a Leyden jar were passed through them, gave strong hydrogen lines when a large jar was interposed. A bulb tube with magnesium wires filled with hydrogen at low pressure gave in one half scarcely any spectrum but the F line of hydrogen, while the other half gave the spectrum of acetylene. They generally found hydrogen lines, and flashes of sodium (no doubt from the glass) in tubes very much exhausted; and they conclude that impurities enter such tubes from sources hitherto unsuspected. Tubes filled with oxygen obtained from silver iodate have been found to give the spectrum of iodine, pointing to the conclusion that chemical reactions occur at very low pressures which are not produced under other circumstances. Generally the authors conclude that the spectrum of a gas in a rarified state affords the most delicate test of its purity, and that it is to the chemical problem of obtaining pure gases that attention needs to be specially directed.

*November* 18, 1878.

PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

The following communication was made to the Society:—

DR ARTHUR SCHUSTER, Ph. D., F.R.A.S. *Some results of the last two total solar eclipses.* (Siam, 1875, and Colorado, 1878.)

Every scientific investigation passes through a preliminary state in which a general survey of the facts is taken and by means of which the most hopeful line for future inquiry is determined. The important investigation on eclipses, which can only be carried on at intervals of several years, for a few minutes at a time, may be said to have just passed through that preliminary stage. The present is therefore a fitting time for a general survey of what has been done, and a discussion of what remains to be done. If, as the title of this paper indicates, I shall refer chiefly to the two last eclipses, it is because on them I can speak from my own experience, and because it is chiefly during the late eclipse that certain changes were proved to go on in the sun's surroundings, which unmistakably point to the line of inquiry which in future will have to be adopted.

1. *Spectroscopic observations.*

The first observations on the spectrum of the corona were made during the eclipse of 1868; but the results were not correctly interpreted until after the eclipse of 1869, during which the

American observers successfully studied the spectrum of the corona. Further facts were established in 1871; we now know that the spectrum of the corona consists:

1. Of a continuous spectrum in which the dark Fraunhofer lines have been faintly seen.

2. Of the spectrum of Hydrogen gas.

3. Of the spectrum of an unknown substance giving a bright green line ($\lambda = 5316$).

Other lines have sometimes been suspected to exist, but nothing definite is known about them.

It will be the object of future eclipse observations to settle the relative intensity of these various spectra as well as the distance to which they reach away from the sun. That the relative brightness of the continuous spectrum to the bright line spectrum varies has been proved during the late eclipse, and the importance of obtaining some numerical data will appear when we come to speak of the polariscopic observations.

The presence of a continuous spectrum indicates the presence of liquid or solid particles; for although we know of many gases and vapours which give continuous spectra at comparatively low temperatures, the presence of polarised light indicates the presence of solid or liquid particles in a finely divided state. It is, as I shall shew, most likely due to matter falling into the sun and being gradually broken up by the heat of the sun.

The existence of the hydrogen lines is not astonishing. The presence of the green line indicates the existence of an unknown gas most likely lighter than hydrogen.

During the last eclipse the first attempt was made by Prof. Eastman, assisted by Mr Pritchett, to gain precise ideas as to height to which the various spectra reach all round the sun. Four directions were taken and the distance in fractions of a solar diameter were estimated at which the spectrum disappeared. The result was rather remarkable, for although the corona was not equal in intensity in the four directions, the spectrum disappeared nearly at the same distance all round the sun. The value of these measurements will appear when compared to similar measurements which no doubt will be made during future eclipses.

It is clear that the spectroscopic observations of the corona would be greatly facilitated if we could succeed in photographing the spectrum of the corona. Attempts in this direction, which to a great extent were successful, have been made during the two last eclipses. The ordinary way of condensing the image of the corona on the slit of a spectroscope, the telescope of which has been replaced by a camera, has hitherto failed. The impossibility of ever succeeding has been asserted; the possibility will be shewn as soon as the experiment has had a fair trial with instruments

which, it is true, will have to be constructed for the purpose, but which it is in our power to construct.

Another instrument however has succeeded, and the photographs obtained by means of it, have even in many respects the advantage over photographs taken by the ordinary method. I speak of an instrument which is termed a prismatic camera, and which consists simply of a spectroscope deprived of its collimator. The parallel ray coming from different parts of the corona are refracted in the ordinary way, and are concentrated on the sensitized plate by means of a lens. If the object is monochromatic a single image will be formed. If the spectrum consists of a series of lines, a series of images will be formed; and, finally, if the spectrum is continuous the image will be drawn out into a band.

The advantage of this method (which is due to Mr Lockyer) consists in the fact that we obtain not only information on the spectrum of a section of the corona as we would if a slit was employed, but of the whole corona. The disadvantage consists in the difficulty to obtain a scale by means of which we can judge in what part of the spectrum the different images are situated. In the Siamese eclipse of 1875 there were luckily some prominences present, which gave us a partly known spectrum on the plate which could be used as reference spectrum. The following results were obtained by means of this instrument.

1°. The lower parts of the corona gave a strong continuous spectrum which left a photographic impression to a wave-length 3530, that is, beyond $N$ and up to a height of 3 minutes from the edge of the sun.

2°. The upper parts of the corona gave a spectrum apparently homogeneous and of a wave-length which seems to coincide with the hydrogen line near $G$, and therefore most likely due to hydrogen.

In addition to this a ring is seen partly round the sun, corresponding to this same hydrogen line. This fact is of importance in connexion with a photograph obtained during the late eclipse by Dr Henry Draper by means of a similar arrangement. No ring shews on Dr Draper's photograph, and this is only one of many facts which tend to shew that the line spectra in the corona this year were uncommonly faint. Prof. Young's testimony on this subject is decisive. The following is taken out of a letter written by him to one of the New York papers and reprinted in Silliman's Journal.

While however there may be room to question the conclusion that the corona this year was uncommonly faint, there can be no question that its spectrum was profoundly modified.

"The bright lines which come from the gaseous constituents were conspicuous in 1869 and in all the subsequent eclipses until the present one, but this year they were so faint as to be seen by

only a few of the observers, while the great majority missed them entirely, seeing only a continuous spectrum......The same constituents appear in the corona as hitherto only in altered proportions, as might have been and was expected by students of Solar Physics. In 1869, 1870 and 1871 the gaseous elements of the corona, the hydrogen and 1474 stuff, whatever that may be...... were in such quantity and condition and rose so high above the solar surface, that their lines were conspicuous in the coronal spectrum and attracted the attention of observers far more forcibly than the feeble continuous spectrum of the light emitted from and reflected by the minute solid and liquid particles which also form an essential element of the corona; at present the condition is reversed. The gases are either too small in quantity or too cool to be conspicuous. The lesson, and it is an important one, is simply, as has been said, that to a certain extent the corona sympathises with the sun-spots."

About the connexion of sun-spots and corona I shall have to say something further on. At present it is important to mention that the almost uniform testimony of every observer goes to confirm Prof. Young's statement as to the extreme faintness of the line spectrum.

Another interesting fact came out through the photographs taken in Siam. The most intense image of the prominences did not correspond to any of the known hydrogen lines; but was due to a line near *H*, the position of which, owing to the small dispersion, could not be exactly determined. Prof. Young suggested that the line might be *H* itself, that is, due to calcium. He had himself observed the calcium lines reversed in the solar spectrum near sun-spots, and indication that calcium reached high up into the chromosphere. During the late eclipse again Prof. Young succeeded in seeing the calcium lines reversed in the chromospheric layer which appeared at the end of the eclipse. The Siamese photographs explain an observation made by Mr Warren de la Rue in the eclipse of 1860. Mr de la Rue obtained the photograph of prominence which was not observed with the naked eye; a result easily explained by the fact that the strongest line of a prominence is at the edge of the visible part of the spectrum.

We are however at present concerned with the sun's corona, and I pass on to the discussion of polariscopic observations.

## 2. *Polariscopic observations.*

The question as to the polarisation of the sun's corona was first started, as far as I know, by Arago. Various attempts to settle the question were made at different times, but the question was only definitely decided when, during the eclipse of 1858, the plane of polarisation was shewn to pass through the centre of the sun.

In 1860 Mr Prazmowski made an important observation indicating that a maximum of polarisation existed a few minutes away from the sun; and that from that point both away from the sun and towards the sun the polarisation decreased. This result was confirmed by Mr Janssen in 1871, and during the same eclipse Mr Winter made a series of measurements which gave a few minutes away from the sun a greater polarisation than close to the edge of the sun. Other and partly contradictory observations exist, but the great mass of them goes only to confirm the results which I mentioned. The existence of polarisation at once suggests the analogous case of our own atmosphere, and it is natural to refer the two cases to a common source. The polarisation of our atmosphere follows the law of polarisation due to the scattering of fine particles. The mathematical theory of this law has been given by Lord Rayleigh, and I have taken his formulae as starting point. The case of our own atmosphere is much simpler than that of the sun's surroundings, because the sun's rays are sensibly parallel when they reach our atmosphere.

I have calculated the amount of polarisation due to the scattering of a particle near the sun. If we consider only a luminous point the plane of polarisation will be that of a plane passing through the luminous point, the scattering point, and the observer's eye. The polarisation will be complete, if the scattered ray is at right angles to the incident ray. It is easy to see that the polarisation will not be complete in any direction when the luminous point is replaced by a luminous sphere, and it is also easy to see that as the point is removed from the sphere the polarisation ought to become more and more complete. The result however is somewhat startling, that close to the sun the light ought not to be polarised at all. According to the formulae which I have obtained the polarisation increases very rapidly as the point is removed from the surface of the sphere; the per centage of light polarised in a ray which makes a right angle with the line joining the centre of the sphere and the scattering point, vanishing on the sphere, as I have said, is 52 per cent. 3 minutes of arc away from the sun; 67 per cent. 6 minutes away; 86 per cent. when the distance is that of the solar radius, and 97 per cent. when it is a diameter and a half. If we look at the corona, however, each line of sight takes in a number of particles; so that even if we look at the edge of the sun the line of sight passes through parts of the corona which are removed from the sun and which therefore partially polarise the light. We cannot therefore tell what the polarisation ought to be along the different lines of sight, unless we know in what way the scattering matter is distributed along the line of sight; but we can, I believe, prove one thing, that in whatever way the amount of scattering matter varies within a

certain distance from the sun, as long as it vanishes nowhere, the polarisation must necessarily increase away from the sun. If the amount of matter is a function only of the distance from the sun, all restrictions fall and it can be strictly proved that in every case the polarisation ought to increase as we move our eyes away from the sun.

As a matter of fact we have seen that after a maximum very near the sun has been reached the polarisation decreases, and we are therefore driven to conclude that, in addition to the light scattered from particles, we have to do with unpolarised light which increases as we move away from the sun and thus removes the preponderance of polarisation. This light cannot be that emitted by the particles themselves, because that cannot increase with increasing distances. It can therefore only be light reflected in the ordinary way from gross matter. The following assumption, I believe, will explain all the facts, and I believe it to be the only one which would explain all the facts. At some distance from the sun the light of the corona is chiefly that due to reflection from solid or liquid bodies sufficiently large to reflect the light according to the ordinary laws of reflection; a part of these bodies attracted by the sun falls into it and in its approach is broken up into such a finely divided state that it polarises the light. The increase of polarisation which we observe is due to the increase in the number of scattering particles brought about by the breaking up of the meteoric matter which is constantly falling into the sun.

Before we pass to another part of the subject let us sum up the conclusions which can be drawn from the spectroscopic and polariscopic observations. We distinguish first the atmosphere proper of the sun, consisting of hydrogen and most likely another unknown gas; we distinguish further a continuous spectrum, which in the outer layer is due to light reflected from solid or liquid matter falling into the sun and is thus gradually broken up. As the matter is broken up the intensity of the continuous spectrum increases, owing to the particles becoming incandescent, at the same time the light becomes partially polarised owing to the scattering of the inner light, which falls on them from the sun's surface.

### 3. *The outline of the corona.*

I wish next to make some remarks on the general outline of the corona. This outline no doubt varies much from eclipse to eclipse, yet it has been remarked that during the late eclipses, the corona was drawn out more in one direction than in another at right angles to it; so that the corona presents the appearance of approximate symmetry round an axis. The direction in which the

corona is drawn out has been variously designated as the sun's equator or as the ecliptic; the two are inclined at an angle 7° only, so that it may be difficult to decide between the two. We may dismiss at once the idea that the greater extension of the corona is due to the centrifugal force caused by the sun's rotation, for the outline is not at all that of a surface which could be produced by such a force. We have then only two explanations to account for it. If we look at the corona simply as an atmosphere of the sun disturbed from inside, we might say that the greatest disturbances take place near the equator, and that therefore the gases might be thrown up higher there. Those however who look partly outside the sun for the explanation of the corona, would say that those meteor streams which cause the appearance of the corona might circulate in a plane which is but little inclined to the ecliptic, as, for instance, the zodiacal light lies approximately in the direction of the sun's equator.

There is, I think, much to be said in favour of this latter view, for the difference between the equatorial and polar regions is much more striking in the outer regions of the corona than it is in the inner regions, and these long streamers of light, extending to a distance of 10 solar diameters, which have been observed during the late eclipse on the top of Pike's Peak, can hardly be due to anything proceeding outward from the sun. There is however in addition to this symmetry in the sun's corona a departure from symmetry which occurs pretty regularly. It is that the corona seems longer and wider on one side of the sun than on the other. Thus, for instance, the corona of 1875 resembled that of 1874, not only through the fact that both times it is extended in the direction of the sun's equator, but also that the branch which lies towards the west is wider than that towards the east. In the eclipse of 1868, the opposite apparently held good. The fact that the two eclipses of 1874 and 1875 took place when the sun and earth were approximately in the same relative position, while in 1868 there was an angle of 120° between the longitudes, suggests that the departure from symmetry to which I have alluded depends on something which is fixed in space.

The connexion which several observers believe to have found between the solar corona and sun-spots has led me to look over the drawings and photographs of former eclipses, and to see whether any difference can be found in the general outline of the corona during periods of maximum or minimum sun-spots. I have been unfortunately restricted to the eclipses of the last 10 years, but there can be no doubt that during these 10 years the corona has undergone a gradual transformation, and that it has now come back again to approximately the same shape it had 10 years ago. In the eclipse of 1868, we find small polar rifts and on each side

of the equator two large rifts, forming what the Siamese in 1875 called fish-tails. In 1869, when the sun-spots had increased to double their number, the form is entirely changed, and we have now large polar rifts which extend as far as the equatorial rifts. In 1870, when the sun-spots had just passed their maximum, it would be difficult to trace any axis of symmetry. In 1871, when the quantity of sun-spots was still large but much smaller, the polar extension was still great, yet there is a distinct axis of symmetry. In 1874, when the quantity of sun-spots was reduced to little more than in 1868, a reversal to the old shape is already distinctly visible, and becomes more apparent in 1875. This last year the general outline of the corona was very similar to that of 1875. Whether this gradual change is accidental or really connected with sun-spots it is too early to assert, but there can be no doubt that the change exists. In trying to discover a reason for such a remarkable connexion between the shape of the corona and the quantity of sun-spots, some ideas forced themselves on me, which, however unproven they may appear at present, are, as far as I know, not contradicted by any facts. I have already mentioned that these long streamers which extend outwards from the sun have to many observers suggested the idea of meteor streams. The way these streamers are distributed shews that their orbits must be very eccentric, and that in their perihelion passage they must pass very near the sun. The great heat at that point, the increased chances of mutual collision, and their entry into the solar atmosphere, must cause many of them to fall into the sun. The local increase of temperature caused by the fall must give rise to currents on the surface of the sun, and may give rise to the cyclones which we call sun-spots. Suppose now that these meteor streams have a period so that every eleven years an increased quantity of meteors passes the perihelion, we should observe every eleven years an increased quantity of sun-spots, and at the same time we should observe a difference in the shape of the corona, which may well be of such a nature as is actually observed.

If I venture to bring this hypothesis forward it is only because it shews the importance of the facts which may be brought to light by eclipse observations.

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Dr Schuster also exhibited to the Society Grant's small calculating machine, for the multiplication of eight figures by eight figures; he explained its construction and compared it with the well-known machine of Thomas de Colmar. Grant's machine is much smaller than Thomas's, but does not perform subtraction directly as is the case with the latter.

*December* 2, 1878.

PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society :—

(1) Dr G. W. ROYSTON-PIGOTT, M.A., M.D., F.R.S., *On a new method of determining the limits of microscopic vision.*

The method I am about to describe, that of forming miniatures, has already been published in the *Philosophical Transactions*[1] and *Proceedings*, but a novel application leads to some interesting results which may not be unworthy of the attention of the Society.

I early became aware that a miniature of a given object, formed by reversing an object-glass, was the severest possible test of the excellence of the glass when examined by another object-glass of high excellence; viz. one in which the rays both central and peripheral were both achromatic and aplanatic, and also in which the mechanical working and centreing were of the highest attainable perfection. These studies were rewarded with some remarkable results, described in a paper on "Circular Solar Spectra" (*Proc. Roy. Soc.* No. 146).

A new application of the method is to form miniatures gradually reduced ten, twenty, fifty, and even a hundred and fifty times, by a reversed objective placed centrally beneath the stage of a microscope, and fixed firmly, yet completely under command, by focussing and rectangular movements of the substage. The miniature is then cautiously brought into the field of view of the observing microscope. See Fig. 1, Plate IV.

By preference I have adopted fine spider lines, which are remarkably true and apparently cylindrical: the liquid gum naturally taking that form on being subjected to contraction and tension. Those I have used most are about the ten-thousandth of an inch in diameter as measured with a delicate micrometer. Dr Brewster measured some as small as $\frac{1}{30000}$th.

There is a good deal of adjustment required to keep the minute field of view presented by the miniature exactly in focus and central. Indifferent glasses, *i.e.* glasses of inferior quality, at once present a perspective obscured by white fog, which denotes spherical aberration, or colour which demonstrates failure in achromatism.

Now high class objectives of half an inch focal length and downwards are furnished with a screw collar for regulating the

[1] Vol. II. 1870.

interval between the front set of lenses, which gives the posterior lenses greater or less power of correcting the aberrations, which change considerably if the distances between object and image vary.

Thus, supposing a tube, used below for giving the miniature, is so short for convenience, that there is only one interval of *four and a half* inches between object and image, the miniature formed (if the glass is adjusted for ten inches) will be found to be violently "*under corrected,*" and in order to get a fine definition in the miniature the interval between the front sets of glasses must be gradually increased by the screw collar until the definition assumes a fine character, resembling the view displayed by a good opera-glass; whilst bad definition will resemble the effect seen through the opera-glass in a London fog by gas-light.

The same precaution is requisite for observing with the microscope itself. In some cases the finest results are obtained with "Immersion lenses," and I have found in a solution of the chloride of gold[1] in glycerine a very beautiful sharpness of definition.

The precise details for managing these arrangements are given in the *Phil. Trans. of the Royal Society* and in the *Proceedings*, especially the latter, which elaborates the matter in a paper on "Circular Solar Spectra," obtained by these means, respecting which it may be permitted to remark here, that the diameter of spurious disks was measured in the microscope: as formed by miniature of the sun's orb examined under a power of one thousand diameters on the stage of the microscope. In this arrangement the whole series of glasses combined in giving the final image, formed at the focus of the eye lens, in a manner precisely similar in all respects to the method now described. But in these elaborated experiments, long continued, the most remarkable phenomenon observed was this: During the sunshine, every bright object line or spot was blurred and obscured, the distant dazzling aerial image of the sun being shaded, but the moment a cloud passed over the sun every minute object in the miniature landscape became perfectly distinct and sharp.

Just the same happens in the system of lenses as now arranged; brilliant lines and disks are enlarged spuriously and obscure dark lines. But dark spider-lines, free from brilliant diffractions, can be beautifully defined on a white ground. The celebrated optical law, deduced independently by Professor Helmholtz, F.R.S., and also by Professor Abbe of Jena (alluded to further on), receives a beautiful illustration by the experiments in the papers cited. The solar disk measured, after its miniature had been effected (1138 divisions of the micrometer being equal to one thousandth of an inch on the stage), nearly $\frac{1}{16000}$ of an inch; whilst the breadth of

[1] 2 grains to the drachm.

the black diffraction ring measured nearly the same as the wave length in Fraunhofer's line *F*, or nearly one-fourth of the diameter of the disk, $\frac{1}{50000}$th. But since the aerial solar image, 1000 inches distant, was on the whole diminished 1000 times, the correct diameter of the stage-disk should have been $\frac{1}{35000}$th of an inch[1]. Whereas the spurious disk measured about twice this amount. This result shews that the Helmholtz law for diffraction was obtained, or rather was sustained, notwithstanding the great number of lenses employed, nearly twenty between the sun and its final image. Moreover the law ceased to operate when the objects were only mildly illuminated. It is reasonable therefore to conclude the miniature image of the spider-lines is sufficiently exempt from spurious enlargement due to brilliant diffraction disks or brilliant lines. Other combinations also confirm this point. Spherical aberration in the glasses causes the lines to be blurred and thickened or rendered invisible. But when an objective possesses the high excellence of presenting an image of a fine fibre sharply, I may say exquisitely defined, when magnified one thousand times, it is plain by reversing the rays a miniature may be formed with great precision which will bear great magnification.

As the experiment requires some practice, it may be well for the observer to commence with easier miniatures, such as hairs or spun glass placed four or five inches below the stage, and if possible illuminated by a plane mirror reflecting daylight, such as a white cloud or grey sky usually affords. An opal globe shading the glare of a parafin flame is also a good back-ground. By degrees, as experience is gained, he may at last ascend to the higher definition, such as that excellently obtained by Messrs Beck's $\frac{1}{20}$ immersion, giving a miniature at seven inches interval diminished 140 times. Figs. 2, 3, 4.

We will now suppose the experiment is ready to be made by the complete adjustment of the coincidence of the axes of the two objectives, the one above and the other below the stage, either "immersion or dry lenses." The preliminary must now be performed of finding the absolute diminution of the miniature for different sets of glasses. And here I should premise that the observing glass in general should not exceed in power one half that of the miniature glass, that is to say, "one quarter" should be used to view the miniature formed by an eighth, unless the latter is of most exquisite excellence, otherwise the spherical errors of the latter will be made horridly visible by the excellence of the observing microscope.

[1] 3-inch lens to heliostat formed image on stage 1000 times less: 100 inches being the distance from the stage. $\left\{ \begin{array}{l} = \dfrac{3 \sin 32'}{1000} = \cdot 000028. \\ = \dfrac{1}{35700} \text{ nearly.} \end{array} \right.$

A spider-line micrometer being inverted below the stage and the lines brought into the field of view, a glass stage-ruled-micrometer was placed so as to intercept the miniature of the webs, the head of the micrometer being divided into one hundred parts, and each revolution representing the hundredth of an inch, the distance of the spider-lines from their miniature image was then carefully measured, and for convenience was limited by a tube strongly built up of screwed "adapters." In the case of Messrs Beck's excellent $\frac{1}{20}$ immersion the tube was increased to seven inches, ten inches being the standard distance for the best performance of the glass. But although it is clear optically, that the miniature will be precisely as much reduced as the objective magnifies in a reversed position of object and image, yet for certainty the micrometer armed as before with the objective was used and tested as a microscope on the same ruled micrometer with the same result. See Fig. 5.

At ten inches the $\frac{1}{20}$th Beck immersion magnifies 200 times, and at seven, 140 times. I insert here a table of some measurements actually made of the reduction in miniature.

| No. | Distance of object from miniature. | Focal length of objective. | Reduction in miniature. | Diameter of $\frac{1}{10000}$th web. | |
|---|---|---|---|---|---|
| (1) | $6\frac{1}{2}$ inches | Ross 1 inch | 6·07 times | $\frac{1}{60700}$th | All these readily distinguishable. |
| (2) | $6\frac{3}{4}$ " | Ross $1\frac{1}{2}$ " | 2·85 " | $\frac{1}{28500}$th | |
| (3) | 6 " | Wray $\frac{1}{2}$ " | 13·40 " | $\frac{1}{134000}$th | |
| (4) | 6 " | Wray $\frac{1}{4}$ " | 29·50 " | $\frac{1}{295000}$th | |
| (5) | $6\frac{1}{4}$ " | A. Ross $\frac{1}{4}$ " | 27·60 " | $\frac{1}{270000}$th | |
| (6) | $6\frac{1}{4}$ " | Powell and Lealand $\frac{1}{8}$ immersion | 58·00 " | $\frac{1}{580000}$th | |
| (7) | $6\frac{1}{4}$ " | Ditto Ditto 1875 | 55·00 " | $\frac{1}{550000}$th | |
| (8) | 7 " | Beck $\frac{1}{20}$ immersion 1878 | 140 " | $\frac{1}{1400000}$th | seen with a little care. |

For convenience the diameter of the webs were supposed to be the $\frac{1}{10000}$ of an inch in diameter: those actually employed were the $\frac{1}{7000}$th and $\frac{1}{8000}$th of an inch, both of which were seen in the miniature cross wires; the calculated diameter of these under the following object-glasses would therefore be

| Distance. | Objective. | Reduction. | Miniature diameters. | |
|---|---|---|---|---|
| $6\frac{1}{4}$ | $\frac{1}{8}$ immersion 1875 | 55 times less | $\frac{1}{7000}$th<br>$\frac{1}{385000}$th | $\frac{1}{8000}$th<br>$\frac{1}{440000}$th |
| 7 | Beck $\frac{1}{20}$ immersion 1878 | 140 times less | $\frac{1}{980000}$th | $\frac{1}{1120000}$th |

In round numbers the smallest spider-line appeared in the $\frac{1}{20}$th Beck miniature $\frac{1}{1000000}$th of an inch in diameter, and it was viewed with a magnificent Powell and Lealand, immersion $\frac{1}{8}$, made expressly for me (price 11 guineas). Now if the microscope magnifies one thousand times, it is interesting to inquire at what visual angle the miniature of the smallest spider-line was seen as a diameter or thickness diminished 140 times.

$$\text{Here } \theta = \frac{\text{arc}}{\text{rad.}} = \frac{\frac{1}{8000} \div 140 \times 1000}{10} \text{ (at 10 inches)}$$

$$= 18\tfrac{1}{2} \text{ seconds nearly.}$$

But I may here state it is by no means necessary to magnify the lines 1000 times to ensure their visibility: 500 will shew them, and then the visual angle is about nine seconds. I may mention that the President of the Royal Microscopic Society, the late Rev. J. B. Reade, F.R.S., informed me by letter he could distinguish single telegraph wires $\frac{1}{4}$ inch in diameter distant 800 yards, at a visual angle, that is, of less than two seconds ($1''{\cdot}85$). This acuteness of vision has been lately surpassed by Mr Slack, P.R.M.S.

He mounted cross hairs in a square frame upon a pole against a grey sky and then against the background of a white wall, and finally viewed them, together with some friends, and lastly observed them with the sun glittering upon them. The distances and visual angles are as under.

| Diameter of hairs ·003 inch. | Distance visible $51\frac{1}{2}$ feet. | Visual angle 1 second (standard). |
|---|---|---|
| ·003 inch | 76 feet | $\frac{3}{4}$ second seen by Mr Slack. |
| ·003 ,, | 123 ,, | glitter distinctly seen, *evidently a spurious diameter.* |

Doubtless, with a good telescope a fine spurious diffraction enlargement would have been descried in the latter case.

As far then as visual angle is concerned, it ought not to seem surprising that a spider-line diminished by miniature 140 times, so as to subtend an angle of nineteen seconds, should be plainly visible in the microscope, even though diminished to the millionth of an inch in diameter by $\frac{1}{20}$ objective of 140° angular aperture.

One or two points are worth referring to as regards the sharpness of the definition. This wholly depends, first upon the quality of the glasses employed, and secondly upon the mode of illumination. For the latter the fairy-like lines of minute tracery are completely hidden by unpropitious glare. The image of the flame of a lamp or of a white sky should be formed *in the plane of the webs.* And when the object-glasses are used deeper for finer miniatures, the corrections of both objectives must be carefully adjusted, so as to produce jet black lines in the field of view. The observer will thus get accustomed to the management of the light and of the glasses. The experiments detailed in the table Nos. (1), (3), (7), will properly introduce the most difficult feat with the Beck $\frac{1}{20}$th.

The result of these experiments to determine the present limits of microscopic vision is confirmed by several considerations, notwithstanding the wide-spread dogma that the limit of microscopic vision is half a wave length; and that only possible with a very large-apertured objective This generally received opinion first originated with Nobert, who quoted Fraunhofer, that the expression $\sin x = \frac{b}{a}$ would become imaginary if the interval between his celebrated lines exceeded a wave length.

Further, Professor Helmholtz has arrived at a beautiful formula for diffraction:

$$\epsilon = \frac{\lambda}{2 \sin \alpha},$$

where $\epsilon$ is the smallest distance visible between two bright lines; $\lambda$ the wave-length, and $\alpha$ the semi-angle of the observing objective,

the limit of which is $\epsilon = \frac{\lambda}{2} = \frac{1}{96000}$ for blue light[1]. Confirmatory of this was the experience of Dr Colonel Woodward of Washington, that he could only succeed in getting clear photographs of the finest lines of diatoms with the blue ray caused by passing solar light through a solution of ammonio sulphate of copper. And in the writer's experience a pale blue glass to tint the illumination greatly improves the experiment with the gossamer threads in question.

Now, however true to the principles of the Undulatory Theory of Light this expression may be, it certainly fails for dark lines. For when the objectives are of the highest order of excellence and adjustment, no diffraction-line is seen by the side of the spider-line miniature. But it is easily raised like a ghost of a line by putting the corrections of the objectives out of order by means of the "screw collars." In fact, the experiment of using only a $\frac{1}{4}$ objective instead of the $\frac{1}{8}$ gives a much larger image of about the $\frac{1}{200000}$th of an inch in diameter; this is easily produced, and is far beyond the supposed limit of microscopic vision. Mr Sorby, F.R.S., in his "President's address to the Royal Microscopic Society," particularly refers to the half-wave length limit for different coloured rays of the solar spectrum. And the Rev. Mr Dallinger has performed the feat of measuring the *flagellum* of monads, as a mean of 250 measures, the result being less than $\frac{1}{200000}$th. Mr Sorby states in his address, No. 87, March 1876, "That the interference fringes, depending upon the essential characters of light itself, deserve far more consideration than has been given to them; that their influence has been entirely overlooked, and that we cannot do better than adopt these principles in forming some conclusion as to the size of the smallest object that could be distinctly seen with a theoretically perfect microscope. Looked at from this point of view alone, with a *dry* lens this could not be less than $\frac{1}{80000}$th of an inch......If it were possible to make use of the blue end alone, lines of $\frac{1}{110000}$ could still be seen, since their shorter waves would not produce obscurity until the size was reduced to $\frac{1}{120000}$th of an inch." Further on he continues, "We must conclude that our instruments do enable us to see intervals so small in relation to the wave length of light, that we can scarcely hope for improvement *as far as the mere visibility of minute objects are concerned,* whatever may be done to improve their performances in other respects," p. 113.

In face of these conclusions, I was fortunate enough to be able to estimate the interval between two spider-lines respectively, the

[1] The lines were the 96000th of an inch apart, as shewn in the photograph by a blue solar ray (sent to the Royal Microscopical Society and to the writer).

$\frac{1}{8000}$th and $\frac{1}{7000}$th in diameter, carefully separated by the micrometer screw three divisions, i.e. $\frac{3}{10000}$th of an inch when diminished 38 times only.

This interval represents the half-diameter of each line $t$, $t'$ plus the space intervening between them, $x$, so that

$$\tfrac{1}{2}t + \tfrac{1}{2}t' + x = \tfrac{3}{10000}.$$

$$\begin{array}{r} 0{\cdot}000300 \\ -\,0{\cdot}000134 \\ \hline \end{array}$$

From which $x = 0{\cdot}0003 - 000134 = 0{\cdot}000166$

Then the miniature being 38 times smaller:—

Apparent interval $\frac{x}{38} = 0{\cdot}00000437 = \frac{1}{230000}$th nearly, the miniature spider-lines being each

$$\frac{1}{38 \times 8000} \text{ and } \frac{1}{38 \times 7000},$$

or $\frac{1}{304000}$ and $\frac{1}{266000}$ respectively.

If now, for the sake of argument, we suppose the miniature lines slightly thickened, then the bright interval between them would be proportionably less than $\frac{1}{230000}$th, a thing which cannot very well be deemed probable. The objection then that in the miniature gossamers we do not see a miniature reduced in the same proportion is disposed of because the lucid interval by such thickening would become smaller than it is possible to be seen according to the diffraction theory. Besides this, it may be added that young, acute sight distinguishes a much smaller interval than that descried by the writer. On another occasion when the miniature was dimished 50 times the visible interval gave five divisions instead of three. But I am not sure that the "back lash" and strain on the micrometer-screw were equable as those of the first example: besides this the utmost care must be taken to ensure centricity of the "wires" to avoid visual parallax, which is sensible in excentric portions of the field of view.

The important formula of Helmholtz, it should be remarked, is totally independent of the number of glasses employed or the power employed. It merely states the conditions upon which a destructive overlapping of brilliant diffractions will totally obscure contiguous lines or disks. It would appear to follow from these circumstances, that, whatever image, formed of the various lenses, is examined instrumentally, the same diffractive obliterations must occur—whether therefore the final image at the eye-piece be formed by a series of miniatures in order, and then magnified up, or by sufficiently small bright objects without miniatures at all, the law equally holds good.

In the case of Nobert's celebrated lines, ruled on glass at the rate of 112,000 per inch, it is impossible to estimate the exact diameter either of the ruled lines or the adjacent separating intervals. The photographs examined by a good lens give a most uncertain appearance. The lines appear about as broad as the intervals in the example lent to me. The interval may therefore roughly be taken at half, i.e. $\frac{1}{220000}$th, very nearly agreeing with the experiment just detailed. But in this case the miniature was only reduced 38 times. The spider-lines most closely, however, resembled the appearance of Nobert's finest band. In the more recondite effect of the miniature by Beck's $\frac{1}{20}$, 140 times reduced, the spider-lines were reduced to four times the tenuity of Nobert's lines—Band XIX.

In support also of the precision of the miniature, it must be considered that the miniature is formed by a cone of rays of the same extreme aperture as the objective, and is received by another of similar large aperture. It is produced under precisely the same aperture and transmitted through a similar one.

Another argument greatly in favour of the miniature result is the following experiment. If two fine wires be placed parallel against the light, they cannot be separated unless the interval is considerably larger than the smallest size visible at a given distance by the unassisted sight.

Least visible separation.

Fig. 1.

Single pin just visible.

Fig. 2.

In reference to this point Dr Jurin found he could discover a pin stuck in a window 40 feet away from him subtending an angle of two or three seconds: but if he placed two pins together he could not distinguish them separated, except they were so far apart as to make an angle of 40 seconds[1]. Taking this remarkable fact into consideration, that a bright interval could not be discerned by Dr Jurin unless it were ten or fifteen times larger than the objects forming it, we may congratulate ourselves on the excellence of modern objectives which enable the eye to discern an interval only four times larger than the diameter of the web miniature, reckoned at one-millionth of an inch.

Referring once more to the formula

$$\epsilon = \frac{\lambda}{2 \sin \alpha},$$

[1] Quoted by Mr Broun, F.R.S. *Proc. R. S.* No. 163, p. 525.

we see that as the semi-aperture diminishes the limit of visibility of bright spaces between dark lines is rapidly changed. At $60^\circ$ aperture $\sin \alpha = \sin 30^\circ = \frac{1}{2}$, $\epsilon = \lambda$ instead of $\frac{1}{2}\lambda$ in the limit.

But it is found that reduction of the aperture of the object-glasses employed in this experiment does not destroy the sharp visibility of the spider-lines; on the contrary, it improves their sharp blackness and definition: the limit being reached at last by deficiency of light, more pronounced in dry objectives. The superiority of immersion lenses over dry ones is very evident in the sharper visibility of these tiny miniatures. The best results are obtained by using a pair of them, a drop of water being inserted between their noses. But for dry lenses, these test lines can be discerned thoroughly well with glasses of all apertures, so long as the observing objective is about half the power of the miniaturing glass, their relative proportions being thus kept up, the actual visual angle $\theta$ will remain the same, and the spider-lines appear of the same delicate tracery as before, notwithstanding the enormous reduction of aperture.

The absolute necessity then for large aperture appears by these experiments to be a delusion, for dark lines, mildly illuminated, ranged exceedingly close together. I possess an excellent $\frac{1}{4}$ triplet by Wray of Highgate, which shews these lines as sharply as can be desired under the same visual diameter, 18 seconds, although the aperture is only $40^\circ$. The same thing can be seen with lower powers.

Dr Carpenter, F.R.S., has long contended for the superiority of small aperture glasses for physiological research, and the visibility of objects subtending no greater angle than Nobert's lines in his finest band by such glasses is an amazing proof of the truth of his opinion.

The extreme obliquity of illumination necessary to develope shadows sufficiently black to represent lines, in the case of a multitude of diatoms, etc., has no doubt originated the extreme angular apertures of some of the most expensive glasses. Tolles in America obtains the very high price for his $\frac{1}{4}$ objective (immersion) of 16 guineas, and this glass resolves the diatom *Amphipleura pellucida*, shewing 100,000 lines per inch. In these complicated structures catching the shadows is the secret of successful resolution. But for the most useful investigations excessive aperture is in many cases, in the writer's opinion, incompetent to deal with minute research.

Hartley Court,
*Nov.* 26, 1878.

(2) Mr W. M. Hicks, M.A., *On the motion of two cylinders in a fluid.*

The investigation relates to the motion of two cylinders surrounded by fluid; more particularly in the cases of (1) one cylinder in an infinite fluid bounded by a plane, (2) two cylinders in an infinite fluid and rigidly connected, (3) one cylinder fixed, and (4) both cylinders free to move generally. The first case corresponds to the motion of two equal cylinders, one moving as the image of the other with respect to the plane. If the cylinder be projected from contact with the plane in a direction perpendicular to it, the limiting velocity as it moves off to an infinite distance is increased in the ratio $\sqrt{\left(\frac{\frac{1}{3}\pi^2+\rho-1}{\rho+1}\right)}$. If it be projected from any point, the future path will have its concavity turned towards the plane, and will turn round and meet the plane or not according as the direction of projection makes an angle with the perpendicular to the plane greater or less than a certain angle $\alpha$, which depends only on the distance of the point from the plane. When the cylinder is projected from contact with the plane the values of $\alpha$ for densities of the cylinder 0, 1, 10, are about $41^\circ\ 22'$; $51^\circ\ 14'$; $70^\circ\ 15'$ respectively.

The case where one cylinder is fixed and the other moves in any manner was also discussed. If they are equal and the moveable one is projected away from the centre of the fixed cylinder, the limiting velocity is $\sqrt{\left(\frac{\frac{1}{4}\pi^2+\rho-1}{\rho+1}\right)}\times$ the initial velocity. In the former case the ratio was $\frac{1}{2}\sqrt{\left(\frac{\frac{1}{3}\pi^2+\rho-1}{\rho+1}\right)}\times$ relative velocity of the cylinder and its image, so that the effect of the constraint is to increase the limiting relative velocity in the two cases in the ratio $\sqrt{\left(3\,\frac{\pi^2+4\rho-4}{\pi^2+3\rho-3}\right)}$. If the cylinder be projected in any way, it will move as if attracted on the whole by the fixed one, and the path will have its concavity turned towards it and will have two asymptotes, whose distance from the centre of the fixed cylinder $=\sqrt{\left(\frac{\rho+P_0}{\rho+1}\right)}\times$ apsidal distance, where $P_0$ is a certain number depending only on this distance. If *e.g.* they touch when nearest, $P_0=\frac{1}{4}\pi^2-1$. If on the contrary both are free to move, and they are projected so that the whole "momentum" of the system is zero, they move as if they repel one another and

the path of one relatively to the other has its convexity towards that other. If they are equal and touch one another at their nearest distance, the distance of the asymptote of the path of one from the centre of the other is $\sqrt{\left(\frac{\frac{1}{6}\pi^2+\rho-1}{\rho+1}\right)}\times$ sum of the radii[1].

*Addition to Mr Glaisher's paper on factor tables*, pp. 99—138.

The accompanying Plate (Plate V) represents, on a reduced scale, the sieves for 13 and 17 described on pp. 131 and 132. The shaded squares are those which are cut out. The 13-sieve is formed of the first thirteen columns of one of the sheets (containing seventy-seven columns) and shows the numbers between 3,000,000 and 3,000,000 + 13 × 300 which have least factors 7, 11 or 13; thus, for example, from the third column we see that

| | | |
|---|---|---|
| 3,000,613 | 3,000,739 | 3,000,823 |
| 3,000,641 | 3,000,767 | 3,000,851 |
| 3,000,683 | 3,000,781 | 3,000,893 |
| 3,000,697 | 3,000,809 | |

have 7 as their factor; that

| | | |
|---|---|---|
| 3,000,679 | 3,000,811 | 3,000,877 |
| 3,000,701 | 3,000,833 | 3,000,899 |

have 11 as their least factor, and that

| | | |
|---|---|---|
| 3,000,647 | 3,000,751 | 3,000,829 |
| 3,000,673 | 3,000,803 | 3,000,881 |

have 13 as their least factor. Of course the numbers such as 3,000,179 for which 7 appears in a shaded square, have 7 as their least factor, and are also divisible by 13; and similarly, when 11 appears in a shaded square, the number has 11 for its least factor and is also divisible by 13.

In the drawing of the 13-sieve the margin, containing the figures 01, 07,... is retained in order to show the arrangement of this column of arguments which is described in the first paragraph of § 20, p. 131; but of course before using the sieve the margin is cut off as in the 17-sieve.

The mode of construction of the sieves is explained on in the last paragraph of p. 134.

[1] See *Quarterly Journal of Mathematics*, Vol. XVI. p. 113.

The list of factor tables given in § 3 (pp. 103—104) is of course not complete; and the smaller tables were intentionally omitted from it. It is worth while, however, on account of its early date, to refer to Cataldi's *Trattato di numeri perfetti*, Bologna, 1603, of which Libri[1] says "ce qu'il y a de plus curieux dans l'ouvrage de Cataldi, c'est une table de diviseurs des nombres jusqu'à 1000." I have not seen Cataldi's work, nor has Professor Cantor, of Heidelberg, to whom I am indebted for the reference to Libri.

I may here correct an erratum in the specimen of a factor table on p. 100, caused by a 5 being out of its place, viz. above 5 there should be a blank instead of 5, and above 10 there should be 2, 5 instead of 2 only.

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The first fourteen printed pages of the factor table for the fourth million were exhibited at the meeting of the Society on October 28, 1878.

[1] *Histoire des sciences mathématiques en Italie*, t. IV. p. 91.

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Fig. 1.

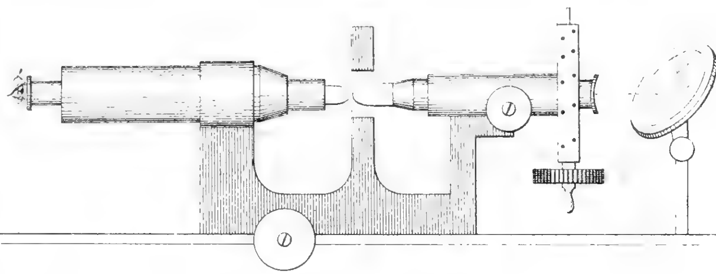



Fig. 2.

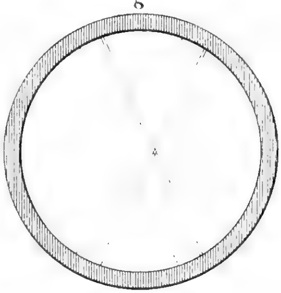
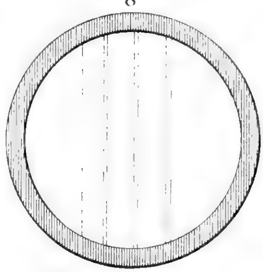
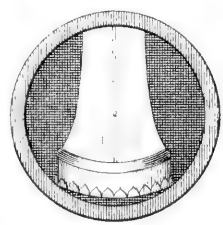


Fig. 3.



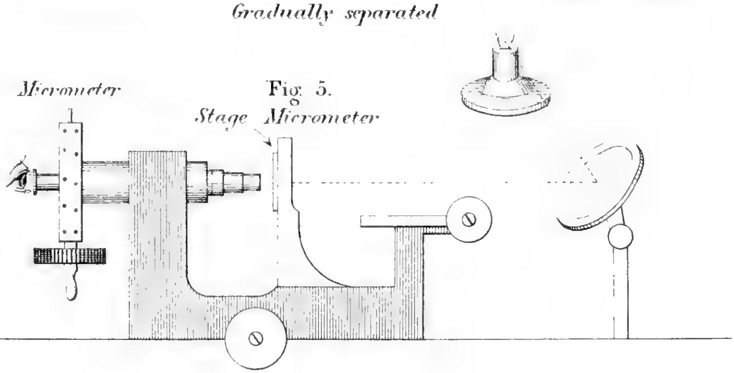
*Spider lines Gradually separated*

Fig. 4.



*Showing spider lines miniatured*

Fig. 5.



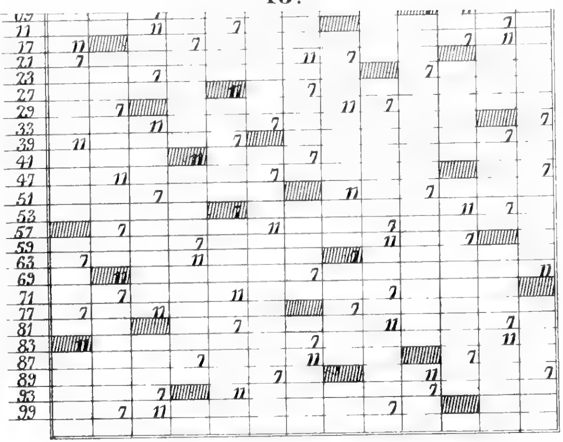

*Micrometer attached as a Microscope to measure power of miniature objective.*

*Dr. Royston Pigott, Del.*

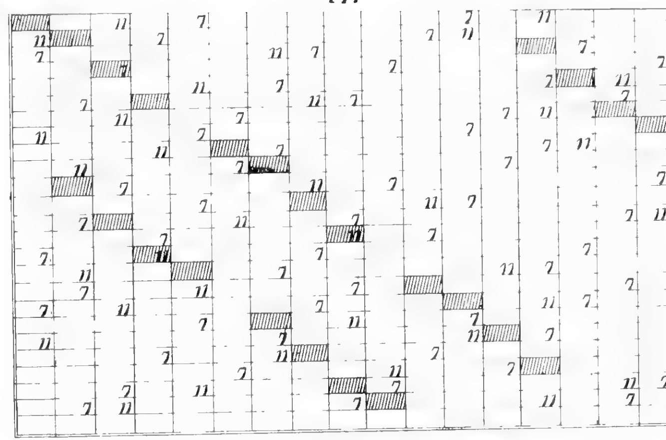
# Diagram illustrating the mode of formation of the Factor Table for the Fourth Million.

*The Sieves for 13 and 17.*

13.

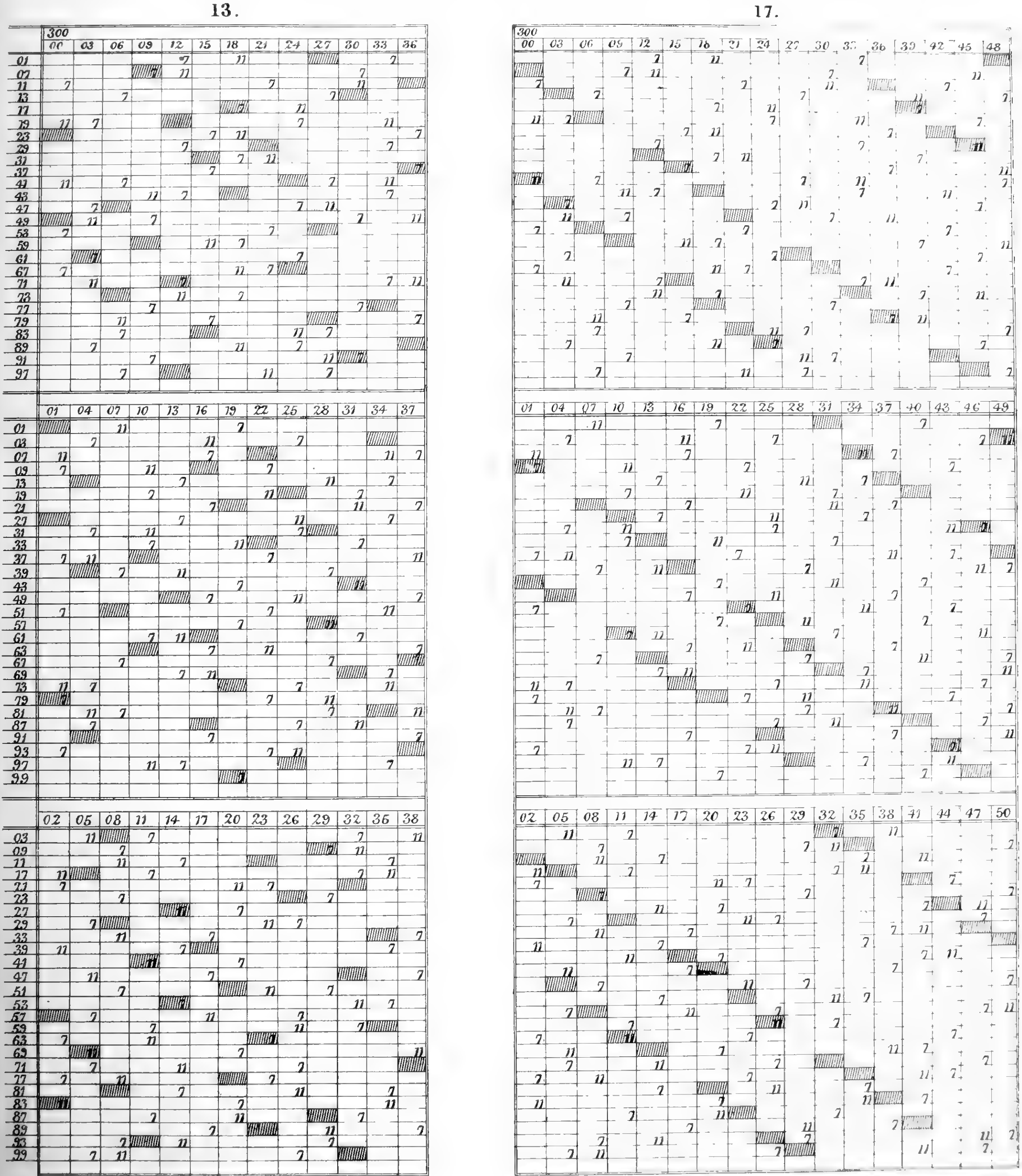


17.



# *Diagram illustrating the mode of formation of the Factor Table for the Fourth Million.*

*The Sieves for 13 and 17.*



# PROCEEDINGS

OF THE

# Cambridge Philosophical Society.

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*February* 10, 1879.

PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

The following communication was made to the Society:—

MR O. FISHER, M.A., *Notes on a mammaliferous deposit at Barrington near Cambridge.*

*February* 24, 1879.

PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

The following communication was made to the Society:—

PROFESSOR CAYLEY, *On the Newton-Fourier imaginary problem.*

The Newtonian process of approximation to the root of a numerical equation $f(u)=0$, consists in deriving from an assumed approximate root $\xi$ a new value $\xi_1 = \xi - \dfrac{f(\xi)}{f'(\xi)}$ which should be a closer approximation to the root sought for: taking the coefficients of $f(u)$ to be real, and also the root sought for, and the assumed value $\xi$, to be each of them real, Fourier investigated the conditions under which $\xi_1$ is in fact a closer approximation. But the question may be looked at in a more general manner: $\xi$ may be any real or imaginary value, and we have to inquire in what cases the series of derived values

$$\xi_1 = \xi - \frac{f(\xi)}{f'(\xi)}, \quad \xi_2 = \xi_1 - \frac{f(\xi_1)}{f'(\xi_1)}, \ldots$$

converge to a root, real or imaginary, of the equation $f(u)=0$. Representing as usual the imaginary value $\xi, = x + iy$, by means of the point whose co-ordinates are $x$, $y$, and in like manner $\xi_1, = x_1 + iy_1$ &c.; then we have a problem relating to an infinite plane; the roots of the equation are represented by points $A, B, C, \ldots$; the value $\xi$ is represented by an arbitrary point $P$; and from this by a determinate geometrical construction we obtain the point $P_1$, and thence in like manner the points $P_2, P_3 \ldots$ which represent the values $\xi_1, \xi_2, \xi_3 \ldots$ respectively. And the problem is to divide the plane into regions, such that starting with a point $P_1$ anywhere in one region we arrive ultimately at the root $A$; anywhere in another region we arrive ultimately at the root $B$; and so on for the several roots of the equation. The division into regions is made without difficulty in the case of a quadric equation, but in the next succeeding case, that of a cubic equation, it is anything but obvious what the division is, and the author had not succeeded in finding it.

*March* 10, 1879.

PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

Mr J. R. HARRIS, Fellow of Clare College, was ballotted for and duly elected a Fellow of the Society, and Mr C. T. HAYCOCK, Exhibitioner of King's College, was ballotted for and duly elected an Associate of the Society.

The following communication was made to the Society:—

Mr J. N. LANGLEY, M.A., *A preliminary account of some phenomena of the central nervous system of the frog.*

The experiments were made on frogs without cerebral hemispheres; the reflex time was at first determined by Türck's method, but this containing many sources of error, a fresh method was later devised with these sources of error as much as possible done away with. The section of either the sciatic nerve, or the cutaneous nerves running to the skin of the back, or of the brachial nerve, caused an increase in the reflex time of the leg with the intact sciatic. So far as the experiments went, the effect was greatest when the sciatic was cut. The effect might be due either to a stimulus causing inhibition set up by the section or by the section removing impulses previously causing inhibition. The latter probably is the case, since in the preliminary exposure of the nerve there is no increase of the reflex-time but usually a diminution, and since weak stimulation of the cut dorsal cutaneous

nerve with dilute acid, as well as weak stimulation of the cut brachial nerve with bile caused a diminution in reflex time. If this is true, it is rendered probable that there are impulses from all portions of the skin surface travelling up the nerves to the central nervous system, each impulse tending to make every nerve centre more irritable, tending to make it discharge efferent impulses more easily. Mr Langley, whilst rejecting the theory of Setschenow of special inhibitory centres, considered that the theory of Goltz and Freusberg required entire alteration as to the mode in which inhibition of reflex action is brought about.

If a frog without cerebral hemispheres be made to jump, in jumping it shuts and opens its eyes, so if either fore or hind limbs be stimulated just so much as to cause a single movement of the limb concerned, the same reflex shutting and opening takes place. If the skin of the fore part of the body be gently touched, the eye is partially shut, sometimes the eye of the stimulated side only without any other movement; in this case the eye is promptly opened if a slight stimulus be applied to the skin of the hinder part of the body, rarely when no movement of the limbs take place.

Again, if the animal is held and then turned over, the eyes more or less completely close, and on turning back, open; if moved at all suddenly in any plane there is a shutting and opening of the eyes. This diminishes, but is not prevented by section of all nerves from the skin surface, or by section of the spinal cord up to second spinal vertebra. The action is probably due to a disturbance of the impressions starting from the semicircular canals.

Some remarks were also made upon the croaking experiment of Goltz and on the so-called cataplexy of frogs.

*March* 24, 1879.

PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

Mr H. M. WARD, Scholar of Christ's College, was ballotted for and duly elected an Associate of the Society.

The following communication was made to the Society:—

Mr A. G. GREENHILL, M.A., *On the rotation of a liquid ellipsoid about its mean axis.*

Jacobi has shewn that an ellipsoid of three unequal axes is a possible form of the free surface of a mass of liquid, rotating as if rigid about one of the principal axes, and in this case the axis of revolution must be the least axis.

Mr Ferrers has shewn that the equations of equilibrium can be

satisfied for an infinite elliptic cylinder, rotating as if rigid about the axis of the cylinder, and here the axis of rotation is the greatest axis, namely the infinite axis of the cylinder.

For if $\omega'$ denote the angular velocity of the cylinder, then the equation for the pressure $p$ is

$$\frac{p}{\rho} - V - \tfrac{1}{2}\omega'^2 (x^2 + y^2) = H, \text{ a constant};$$

the axis of the cylinder being the axis of $z$, and the gravitation potential

$$V = \text{constant} - 2\pi\rho \frac{bx^2 + ay^2}{a+b},$$

when $a$, $b$ are the semi-axes of the elliptic section of the cylinder.

Hence the surfaces of equal pressure are the similar cylinders

$$\left(\frac{4\pi\rho b}{a+b} - \omega'^2\right) x^2 + \left(\frac{4\pi\rho a}{a+b} - \omega'^2\right) y^2 = \text{constant},$$

and these are similar to the outer surface of the cylinder, and a free surface is therefore possible if

$$a^2\left(\frac{4\pi\rho b}{a+b} - \omega'^2\right) = b^2\left(\frac{4\pi\rho a}{a+b} - \omega'^2\right),$$

or

$$\omega'^2 = 4\pi\rho \frac{ab}{(a+b)^2}.$$

If the motion in the elliptic cylinder had been generated from rest in a frictionless liquid, we should have the velocity function $\phi$ for an angular velocity $\omega$ about the axis of the cylinder given by

$$\phi = \omega \frac{a^2 - b^2}{a^2 + b^2} xy,$$

and therefore

$$\frac{p}{\rho} - V + \frac{\partial\phi}{\partial t} + \tfrac{1}{2}\left(\frac{\partial\phi}{\partial x}\right)^2 + \tfrac{1}{2}\left(\frac{\partial\phi}{\partial y}\right)^2 = H,$$

giving the pressure $p$.

Here $xy$ denotes a point, *fixed in space*, and therefore

$$\frac{dx}{dt} - y\omega = 0, \quad \frac{dy}{dt} + x\omega = 0,$$

$$\frac{d\phi}{dt} = \omega \frac{a^2 - b^2}{a^2 + b^2}\left(\frac{dx}{dt} y + x \frac{dy}{dt}\right)$$

$$= \omega^2 \frac{a^2 - b^2}{a^2 + b^2}(y^2 - x^2);$$

and $$\left(\frac{\partial\phi}{\partial x}\right)^2+\left(\frac{\partial\phi}{\partial y}\right)^2=\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2(x^2+y^2);$$

therefore

$$\frac{\partial\phi}{\partial t}+\tfrac{1}{2}\left(\frac{\partial\phi}{\partial x}\right)^2+\tfrac{1}{2}\left(\frac{\partial\phi}{\partial y}\right)^2,$$

$$=-\tfrac{1}{2}\omega^2\frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}x^2+\tfrac{1}{2}\omega^2\frac{(a^2-b^2)(3a^2+b^2)}{(a^2+b^2)^2}y^2,$$

and therefore

$$\frac{p}{\rho}+\tfrac{1}{2}\left\{\frac{4\pi\rho b}{a+b}-\omega^2\frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}\right\}x^2$$

$$+\tfrac{1}{2}\left\{\frac{4\pi\rho a}{a+b}+\omega^2\frac{(a^2-b^2)(3a^2+b^2)}{(a^2+b^2)^2}\right\}y^2=\text{constant.}$$

The surfaces of equal pressure are therefore the similar cylinders

$$\left\{\frac{4\pi\rho b}{a+b}-\omega^2\frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}\right\}x^2$$

$$+\left\{\frac{4\pi\rho a}{a+b}+\omega^2\frac{(a^2-b^2)(3a^2+b^2)}{(a^2+b^2)^2}\right\}y^2=\text{constant};$$

and these are similar to the outer surface, and a free surface is therefore possible if

$$a^2\left\{\frac{4\pi\rho b}{a+b}-\omega^2\frac{(a^2-b^2)(a^2+3b)}{(a^2+b^2)^2}\right\}$$

$$=b^2\left\{\frac{4\pi\rho a}{a+b}+\omega^2\frac{(a^2-b^2)(3a^2+b^2)}{(a^2+b^2)^2}\right\},$$

or $$\omega^2=4\pi\rho\frac{ab(a^2+b^2)^2}{(a+b)^2(a^2+6a^2b^2+b^2)}.$$

More generally, if the cylinder of liquid had been rotating as if rigid with angular velocity $\omega'$, and an additional angular velocity $\omega$ had been communicated to the cylinder, then the components $u$, $v$ of the velocity at the point $xy$, the axes of $x$ and $y$ being taken as the axes of the elliptic section, and therefore rotating with angular velocity $\omega+\omega'$, are

$$u=\omega\frac{a^2-b^2}{a^2+b^2}y-\omega'y,$$

$$v=\omega\frac{a^2-b^2}{a^2+b^2}x+\omega'x,$$

the molecular rotation at every point being $\omega$.

The dynamical equations are

$$\frac{1}{\rho}\frac{\partial p}{\partial x}-\frac{\partial V}{\partial x}+\frac{\partial u}{\partial t}+u\frac{\partial u}{\partial x}+u\frac{\partial u}{\partial y}=0,$$

$$\frac{1}{\rho}\frac{\partial p}{\partial y}-\frac{\partial V}{\partial y}+\frac{\partial v}{\partial t}+u\frac{\partial v}{\partial x}+v\frac{\partial v}{\partial y}=0.$$

Now since the axes are rotating with angular velocity $\omega+\omega'$,

$$\frac{\partial u}{\partial t}=\left(\omega\frac{a^2-b^3}{a^2+b^2}-\omega'\right)\dot{y}-(\omega+\omega')v,$$

and $\dot{y}=(\omega+\omega')x$, since $xy$ refers to a point fixed in space. Therefore

$$\frac{\partial u}{\partial t}=-2\omega(\omega+\omega')\frac{a^2-b^2}{a^2+b^2}x.$$

Similarly,

$$\frac{\partial v}{\partial t}=2\omega(\omega+\omega')\frac{a^2-b^2}{a^2+b^2}y.$$

And
$$\frac{\partial u}{\partial x}=0,\quad v\frac{\partial u}{\partial y}=\left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2-\omega'^2\right\}x,$$

$$\frac{\partial v}{\partial y}=0,\quad u\frac{\partial v}{\partial x}=\left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2-\omega'^2\right\}y.$$

Therefore the dynamical equations become

$$\frac{1}{\rho}\frac{\partial p}{\partial x}-\frac{\partial V}{\partial x}-2\omega(\omega+\omega')\frac{a^2-b^2}{a^2+b^2}x+\left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2-\omega'^2\right\}x=0,$$

$$\frac{1}{\rho}\frac{\partial p}{\partial y}-\frac{\partial V}{\partial y}+2\omega(\omega+\omega')\frac{a^2-b^2}{a^2+b^2}y+\left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2-\omega'^2\right\}y=0;$$

and therefore, integrating,

$$\frac{p}{\rho}-V-\omega(\omega+\omega')\frac{a^2-b^2}{a^2+b^2}(x^2-y^2)+$$
$$\tfrac{1}{2}\left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2-\omega'^2\right\}(x^2+y^2)=H,$$

a constant.

The surfaces of equal pressure are therefore similar cylinders, and similar to the external surface if

$$a^2\left\{\frac{4\pi\rho b}{a+b}-2\omega(\omega+\omega')\frac{a^2-b^2}{a^2+b^2}+\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2-\omega'^2\right\}$$
$$=b^2\left\{\frac{4\pi\rho a}{a+b}+2\omega(\omega+\omega')\frac{a^2-b^2}{a^2-b^2}+\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2-\omega'^2\right\},$$

or
$$4\pi\rho ab\frac{a-b}{a+b}$$
$$=2\omega(\omega+\omega')(a^2-b^2)-\left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2-\omega'^2\right)(a^2-b^2)$$

or
$$(\omega+\omega')^2+\frac{4a^2b^2}{(a^2+b^2)^2}\omega'^2=\frac{4\pi\rho ab}{(a+b)^2}.$$

As a particular case, we may have the elliptic cylinder stationary, and then
$$\omega+\omega'=0,$$
and
$$\omega^2=\pi\rho\frac{(a^2+b^2)^2}{ab(a+b)^2}.$$

In the particular case, considered by Kirchoff (*Vorlesungen über mathematische Physik*, p. 263),
$$\omega+\omega'=\lambda,\quad \omega'=\zeta,$$
and therefore
$$\frac{\omega+\omega'}{\omega'}=\frac{2ab}{(a+b)^2}$$
or
$$\frac{\omega}{\omega'}=-\frac{a^2+b^2}{(a+b)^2}.$$

Relatively to the cylinder, each particle of liquid describes an ellipse; for if $xy$ refers to a particle of liquid, the axes being the principal axes of the section,
$$\frac{dx}{dt}-y\omega=\omega\frac{a^2-b^2}{a^2+b^2}y,$$
$$\frac{dy}{dt}+x\omega=\omega\frac{a^2-b^2}{a^2+b^2}x,$$
or
$$\frac{dx}{dt}=\omega\frac{2a^2}{a^2+b^2}y,\qquad \frac{dy}{dt}=-\omega\frac{2b^2}{a^2+b^2}x.$$
Therefore
$$\frac{x}{a^2}\frac{dx}{dt}+\frac{y}{b^2}\frac{dx}{dt}=0,$$
or
$$\frac{x^2}{a^2}+\frac{y^2}{b^2}=\text{constant},$$
and if initially $x=ma\cos\phi$, $y=mb\sin\phi$, then at the time $t$,
$$x=ma\cos\left(\frac{2ab}{a^2+b^2}\omega t-\phi\right),$$
$$y=-mb\sin\left(\frac{2ab}{a^2+b^2}\omega t-\phi\right).$$

In the general case, therefore, when the axes of the cylinder are revolving with angular velocity $\omega + \omega'$, the co-ordinates $x$, $y$ of a particle initially at a point whose co-ordinates are $ma \cos \phi$, $mb \sin \phi$, are given at time $t$ by

$$x = ma \cos \left(\frac{2ab}{a^2+b^2}\omega t - \phi\right) \cos (\omega + \omega') t$$

$$+ mb \sin \left(\frac{2ab}{a^2+b^2}\omega t - \phi\right) \sin (\omega + \omega') t$$

$$= \tfrac{1}{2} m (a+b) \cos \left(\omega' t + \frac{(a-b)^2}{a^2+b^2}\omega t + \phi\right)$$

$$+ \tfrac{1}{2} m (a-b) \cos \left(\omega' t + \frac{(a+b)^2}{a^2+b^2}\omega t - \phi\right),$$

and

$$y = ma \cos \left(\frac{2ab}{a^2+b^2}\omega t - \phi\right) \sin (\omega + \omega') t$$

$$- mb \sin \left(\frac{2ab}{a^2+b^2}\omega t - \phi\right) \cos (\omega + \omega') t$$

$$= \tfrac{1}{2} m (a+b) \sin \left(\omega' t + \frac{(a-b)^2}{a^2+b^2}\omega t + \phi\right)$$

$$+ \tfrac{1}{2} m (a-b) \sin \left(\omega' t + \frac{(a+b)^2}{a^2+b^2}\omega t - \phi\right).$$

The particles of the liquid therefore describe pericycloids, which (1) when $\dfrac{\omega'}{\omega} = \dfrac{a^2-b^2}{a^2+b^2}$ are epicycloids; (2) when $\omega + \omega' = 0$ are ellipses; (3) when $\omega = 0$ are circles; (4) when

$$\omega + \omega' = \pm \frac{2ab}{a^2+b^2}\omega$$

are circles; the particular case considered by Kirchoff.

In the Göttingen Transactions for 1859 and 1860 Lejeune Dirichlet and Riemann have attacked the general problem of the motion of a mass of liquid in the shape of an ellipsoid, and discussed the particular cases where a free surface is possible, the liquid being under the action of the mutual gravitation of the particles.

If we take the liquid, supposed frictionless, filling the ellipsoid $\dfrac{x^2}{a^2} + \dfrac{y^2}{b^2} + \dfrac{z^2}{c^2} = 1$, originally at rest, and if at any instant the component angular velocities about the axes be $\omega_1$, $\omega_2$, $\omega_3$; then the velocity function

$$\phi = \omega_1 \frac{b^2 - c^2}{b^2 + c^2} yz + \omega_2 \frac{c^2 - a^2}{c^2 + a^2} zx + \omega_3 \frac{a^2 - b^2}{a^2 + b^2} xy,$$

and therefore if $M$ denote the mass of the liquid, the kinetic energy

$$T = \tfrac{1}{2} M \left\{ \frac{(b^2 - c^2)^2}{5(b^2 + c^2)} \omega_1^{\,2} + \frac{(c^2 - a^2)^2}{5(c^2 + a^2)} \omega_2^{\,2} + \frac{(a^2 - b^2)^2}{5(a^2 + b^2)} \omega_3^{\,2} \right\},$$

and the liquid is therefore kinetically equivalent to a mass $M$ of principal moments of inertia

$$M \frac{(b^2 - c^2)^2}{5(b^2 + c^2)}, \quad M \frac{(c^2 - a^2)^2}{5(c^2 + a^2)}, \quad M \frac{(a^2 - b^2)^2}{5(a^2 + b^2)}.$$

Consequently, if no external forces act, and the inertia of the case be neglected, Euler's equations of motion of the ellipsoid become

$$\dot{\omega}_1 = \frac{\dfrac{(c^2 - a^2)^2}{c^2 + a^2} - \dfrac{(a^2 - b^2)^2}{a^2 + b^2}}{\dfrac{(b^2 - c^2)^2}{b^2 + c^2}} \omega_2 \omega_3$$

$$= \frac{b^2 + c^2}{b^2 - c^2} \left\{ \frac{4a^4}{(c^2 + a^2)(a^2 + b^2)} - 1 \right\} \omega_2 \omega_3,$$

with two similar equations.

If $p$ denote the pressure of the liquid at any point $xyz$, $\rho$ the density, $q$ the velocity, and $V$ the gravitation potential of the ellipsoid, then we have

$$\frac{p}{\rho} - V + \frac{\partial \phi}{\partial t} + \tfrac{1}{2} q^2 = H, \text{ a constant.}$$

Now
$$\frac{\partial \phi}{\partial t} = \dot{\omega}_1 \frac{b^2 - c^2}{b^2 + c^2} yz + \omega_1 \frac{b^2 - c^2}{b^2 + c^2} (\dot{y} z + y \dot{z}) + \ldots$$

$$= \left\{ \frac{4a^4}{(c^2 + a^2)(a^2 + b^2)} - 1 \right\} \omega_2 \omega_3 yz$$

$$+ \frac{b^2 - c^2}{b^2 + c^2} \{ -\omega_1^{\,2} (y^2 - z^2) - \omega_3 \omega_1 zx + \omega_1 \omega_2 xy \}$$

$$+ \left\{ \frac{4b^4}{(a^2 + b^2)(b^2 + c^2)} - 1 \right\} \omega_3 \omega_1 yz$$

$$+ \frac{c^2 - a^2}{c^2 + a^2} \{ -\omega_2^{\,2} (z^2 - x^2) - \omega_1 \omega_2 xy + \omega_2 \omega_3 xy \}$$

$$+ \left\{ \frac{4c^4}{(b^2 + c^2)(c^2 + a^2)} - 1 \right\} \omega_1 \omega_2 xy$$

$$+ \frac{a^2 - b^2}{a^2 + b^2} \{ -\omega_3^{\,2} (x^2 - y^2) - \omega_2 \omega_3 xy + \omega_3 \omega_1 zx \}$$

$$= -\frac{b^2-c^2}{b^2+c^2}\omega_1^2(y^2-z^2) - \frac{c^2-a^2}{c^2+a^2}\omega_2(z^2-x^2) - \frac{a^2-b^2}{a^2+b^2}\omega_3^2(x^2-y^2)$$

$$-\frac{c^2-a^2}{c^2+a^2}\frac{a^2-b^2}{a^2+b^2}\omega_2\omega_3 yz - \frac{a^2-b^2}{a^2+b^2}\frac{b^2-c^2}{b^2+c^2}\omega_3\omega_1 zx$$

$$-\frac{b^2-c^2}{b^2+c^2}\frac{c^2-a^2}{c^2+a^2}\omega_1\omega_2 xy.$$

(We have
$$\dot{x} - y\omega_3 + z\omega_2 = 0,$$
$$\dot{y} - z\omega_1 + x\omega_3 = 0,$$
$$\dot{z} - x\omega_2 + y\omega_1 = 0,$$
because $xyz$ is here taken to denote a point fixed in space.)

Therefore

$$\frac{\partial\phi}{\partial t} + \tfrac{1}{2}q^2$$

$$= x^2\left\{\frac{c^2-a^2}{c^2+a^2}\omega_2^2 - \frac{a^2-b^2}{a^2+b^2}\omega_3^2 + \tfrac{1}{2}\left(\frac{c^2-a^2}{c^2+a^2}\right)^2\omega_2^2 + \tfrac{1}{2}\left(\frac{a^2-b^2}{a^2+b^2}\right)^2\omega_3^2\right\}$$
$$+ y^2\{\ldots\ldots\ldots\ldots\} + z^2\{\ldots\ldots\ldots\ldots\};$$

the terms involving $\omega_2\omega_3 yz_1$, $\omega_3\omega_1 zx_1$, $\omega_1\omega_2 xy$ disappearing; and therefore

$$\frac{\partial\phi}{\partial t} + \tfrac{1}{2}q^2 = \tfrac{1}{2}x^2\left\{\frac{(c^2-a^2)(3c^2+a^2)}{(c^2+a^2)^2}\omega_2^2 - \frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}\omega_3^2\right\}$$
$$+ \tfrac{1}{2}y^2\{\ldots\ldots\ldots\ldots\} + \tfrac{1}{2}z^2\{\ldots\ldots\ldots\ldots\},$$

and

$$V = \tfrac{3}{4}M\int_0^\infty \frac{d\lambda}{\sqrt{a^2+\lambda\,.\,b^2+\lambda\,.\,c^2+\lambda}}\left(1 - \frac{x^2}{a^2+\lambda} - \frac{y^2}{b^2+\lambda} - \frac{z^2}{c^2+\lambda}\right)$$
$$= \text{constant} - \tfrac{1}{2}Ax^2 - \tfrac{1}{2}By^2 - \tfrac{1}{2}Cz^2 \text{ suppose.}$$

The surfaces of equal pressure at any instant are therefore the similar ellipsoids

$$\left\{A + \frac{(c^2-a^2)(3c^2+a^2)}{(c^2+a^2)^2}\omega_2^2 - \frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}\omega_3^2\right\}x^2$$
$$+\left\{B + \frac{(a^2-b^2)(3a^2+b^2)}{(a^2+b^2)^2}\omega_3^2 - \frac{(b^2-c^2)(b^2+3c^2)}{(b^2+c^2)^2}\omega_1^2\right\}y^2$$
$$+\left\{C + \frac{(b^2-c^2)(3b^2+c^2)}{(b^2+c^2)^2}\omega_1^2 - \frac{(c^2-a^2)(c^2+3a^2)}{(c^2+a^2)^2}\omega_2^2\right\}z^2$$
$$= \text{constant.}$$

The shape of the surfaces of equal pressure therefore changes every instant, unless we can make

$$\frac{(c^2-a^2)(3c^2+a^2)}{(c^2+a^2)^2}\omega_2^{\,2}-\frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}\omega_3^{\,2},$$

and the two similar expressions constant.

This can only be the case when the ellipsoid is rotating about a principal axis, say the axis of $z$; then $\omega_1=0$, $\omega_2=0$, and $\omega_3=\omega$ suppose, and we can make the surfaces of equal pressure similar to the external surface by putting

$$\begin{aligned} &a^2\left\{A-\frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}\omega^2\right\}\\ &=b^2\left\{B+\frac{(a^2-b^2)(3a^2+b^2)}{(a^2+b^2)^2}\omega^2\right\}\\ &=c^2C,\end{aligned}$$

or
$$\omega^2=\frac{a^2A-c^2C}{a^2\dfrac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}}=\frac{c^2C-b^2B}{b^2\dfrac{(a^2-b^2)(3a^2+b^2)}{(a^2+b^2)^2}}.$$

Hence $c$ must be the *mean* axis, and we can therefore have an ellipsoid of liquid rotating in equilibrium about the mean axis, the motion of the liquid particles being that which would be generated in the liquid from rest.

Therefore
$$\frac{a^2A-c^2C}{a^2(a^2+3b^2)}=\frac{c^2C-b^2B}{b^2(3a^2+b^2)},$$

or
$$a^2b^2(3a^2+b^2)A+a^2b^2(a^2+3b^2)B-(a^4+6a^2b^2+b^4)c^2C=0,$$

r

$$\int_0^\infty\frac{a^2b^2(3a^2+b^2)(b^2+\lambda)(c^2+\lambda)+a^2b^2(a^2+3b^2)(c^2+\lambda)(a^2+\lambda)-(a^4+6a^2b^2+b^4)c^2(a^2+\lambda)(b^2+\lambda)}{P^3}d\lambda=0,$$

where
$$P^2=(a^2+\lambda)(b^2+\lambda)(c^2+\lambda),$$

of which the numerator $N$

$$\begin{aligned}&=\lambda^2\{4a^2b^2(a^2+b^2)-c^2(a^4+6a^2b^2+b^4)\}\\ &+\lambda\{a^2b^2(3a^2+b^2)(b^2+c^2)+a^2b^2(a^2+3b^2)(c^2+a^2)\\ &\qquad -c^2(a^2+b^2)(a^4+6a^2b^2+b^4)\}.\end{aligned}$$

If we put $c^2 = a^2$,

$$N = -a^2(a^2-b^2)(a^2+3b^2)(a^2+\lambda)\lambda;$$

and if we put $c^2 = b^2$,

$$N = b^2(a^2-b^2)(3a^2+b^2)(b^2+\lambda)\lambda;$$

and therefore for some value of $c$ between $a$ and $b$, the integral

$$\int_0^\infty \frac{N}{P^3}\,d\lambda = 0.$$

If the ellipsoid had been rotating as if rigid with angular velocity $\omega'$, we should have the equation

$$\frac{dp}{\rho} - dV - \omega'^2 x\,dx - \omega'^2 y\,dy = 0,$$

or

$$\frac{p}{\rho} - V - \tfrac{1}{2}\omega'^2(x^2+y^2) = H;$$

and the equation of the surfaces of equal pressure would be

$$(A-\omega'^2)x^2 + (B-\omega'^2)y^2 + C = \text{constant};$$

and therefore if a free surface can exist, these ellipsoids must be similar to the external surface, and

$$a^2(A-\omega'^2) = b^2(B-\omega'^2) = c^2C,$$

or

$$\omega'^2 = \frac{a^2A - c^2C}{a^2} = \frac{b^2B - c^2C}{b^2},$$

and therefore $c$ must be the least axis; this is the case considered by Jacobi.

In Jacobi's case

$$a^2b^2(A-B) + (a^2-b^2)c^2C = 0,$$

or

$$a^2b^2\int_0^\infty \frac{d\lambda}{(a^2+\lambda)(b^2+\lambda)P} - c^2\int_0^\infty \frac{d\lambda}{(c^2+\lambda)P} = 0,$$

where

$$P^2 = (a^2+\lambda)(b^2+\lambda)(c^2+\lambda),$$

or

$$\int_0^\infty \frac{(a^2b^2 - a^2c^2 - b^2c^2)\lambda - c^2\lambda^2}{P^3}\,d\lambda = 0.$$

If $c = 0$, the integral is positive, and if $c^2 = \dfrac{a^2b^2}{a^2+b^2}$, the integral is negative; consequently $c$ must have some value between $0$ and $\dfrac{ab}{\sqrt{a^2+b^2}}$.

To reduce the relation

$$\frac{a^2A - c^2C}{a^2} = \frac{b^2B - c^2C}{b^2},$$

we must put

$$a^2 + \lambda = (a^2 - c^2)\frac{1}{\operatorname{sn}^2 u},$$

$$b^2 + \lambda = (a^2 - c^2)\frac{\operatorname{dn}^2 u}{\operatorname{sn}^2 u},$$

$$c^2 + \lambda = (a^2 - c^2)\frac{\operatorname{cn}^2 u}{\operatorname{sn}^2 u},$$

where

$$k^2 = \frac{a^2 - b^2}{a^2 - c^2}, \quad a > b > c;$$

and

$$a^2 = (a^2 - c^2)\frac{1}{\operatorname{sn}^2 \alpha},$$

$$b^2 = (a^2 - c^2)\frac{\operatorname{dn}^2 \alpha}{\operatorname{sn}^2 \alpha},$$

$$c^2 = (a^2 - c^2)\frac{\operatorname{cn}^2 \alpha}{\operatorname{sn}^2 \alpha};$$

therefore

$$\operatorname{cn}^2\alpha = \frac{c^2}{a^2}, \qquad \operatorname{dn}^2\alpha = \frac{b^2}{a^2}; \text{ and}$$

$$A = \frac{3M}{(a^2 - c^2)^{\frac{3}{2}}}\int_0^\alpha \operatorname{sn}^2 u\, du,$$

$$B = \frac{3M}{(a^2 - c^2)^{\frac{3}{2}}}\int_0^\alpha \frac{\operatorname{sn}^2 u}{\operatorname{dn}^2 u}\, du,$$

$$C = \frac{3M}{(a^2 - c^2)^{\frac{3}{2}}}\int_0^\alpha \frac{\operatorname{sn}^2 u}{\operatorname{cn}^2 u}\, du.$$

Therefore to determine $\alpha$,

$$\int_0^\alpha \operatorname{sn}^2 u\, du - \operatorname{cn}^2\alpha \int_0^\alpha \frac{\operatorname{sn}^2 u}{\operatorname{cn}^2 u}\, du = \int_0^\alpha \frac{\operatorname{sn}^2 u}{\operatorname{dn}^2 u}\, du - \frac{\operatorname{cn}^2\alpha}{\operatorname{dn}^2\alpha}\int_0^\alpha \frac{\operatorname{sn}^2 u}{\operatorname{cn}^2 u}\, du,$$

or

$$\int_0^\alpha \operatorname{sn}^2 u\, du + k^2\frac{\operatorname{sn}^2\alpha\operatorname{cn}^2\alpha}{\operatorname{dn}^2\alpha}\int_0^\alpha \frac{\operatorname{sn}^2 u}{\operatorname{cn}^2 u}\, du - \int_0^\alpha \frac{\operatorname{sn}^2 u}{\operatorname{dn}^2 u}\, du = 0,$$

or $$\frac{1}{k^2}\left(1-\frac{E}{K}\right)\alpha - \frac{1}{k^2}Z\alpha + \frac{k^2\,\mathrm{sn}^2\alpha\,\mathrm{cn}^2\alpha}{k'^2\,\mathrm{dn}^2\alpha}\left(\frac{\mathrm{sn}\,\alpha\,\mathrm{dn}\,\alpha}{\mathrm{cn}\,\alpha} - \frac{E}{K}\alpha - Z\alpha\right)$$
$$-\frac{1}{k^2}\left(-\frac{k^2}{k'^2}\frac{\mathrm{sn}\,\alpha\,\mathrm{cn}\,\alpha}{\mathrm{dn}\,\alpha} + \frac{1}{k'^2}\frac{E}{K}\alpha + \frac{1}{k'^2}Z\alpha - \alpha\right) = 0.$$

If now an additional angular velocity $\omega$ be communicated to the ellipsoid about the axis of $z$, then we shall have

$$u = \omega\frac{a^2-b^2}{a^2+b^2}y - \omega' y,$$
$$v = \omega\frac{a^2-b^2}{a^2+b^2}x + \omega' x,$$
$$w = 0;$$

the axes of $x$ and $y$ rotating with angular velocity $\omega+\omega'$, and the dynamical equations

$$\frac{1}{\rho}\frac{\partial p}{\partial x} + Ax + \frac{\partial u}{\partial t} + u\frac{\partial u}{\partial x} + v\frac{\partial u}{\partial y} = 0,$$
$$\frac{1}{\rho}\frac{\partial p}{\partial y} + By + \frac{\partial v}{\partial t} + u\frac{\partial v}{\partial x} + v\frac{\partial v}{\partial y} = 0,$$
$$\frac{1}{\rho}\frac{\partial p}{\partial z} + Cz = 0,$$

become

[since $$\frac{\partial u}{\partial t} = \left(\omega\frac{a^2-b^2}{a^2+b^2} - \omega'\right)\dot{y} - (\omega+\omega')v$$
$$= -2\omega\frac{a^2-b^2}{a^2+b^2}(\omega+\omega')x,$$
$$\frac{\partial v}{\partial t} = 2\omega\frac{a^2-b^2}{a^2+b^2}(\omega+\omega')y,$$
$$\frac{\partial u}{\partial x} = 0; \quad v\frac{\partial u}{\partial y} = \left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2 - \omega'^2\right\}x,$$
$$\frac{\partial v}{\partial y} = 0; \quad u\frac{\partial v}{\partial x} = \left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2 - \omega'^2\right\}y \Bigg];$$

$$\frac{1}{\rho}\frac{\partial p}{\partial x} + Ax - 2\omega(\omega+\omega')\frac{a^2-b^2}{a^2+b^2}x + \left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2 - \omega'^2\right\}x = 0,$$
$$\frac{1}{\rho}\frac{\partial p}{\partial y} + By + 2\omega(\omega+\omega')\frac{a^2-b^2}{a^2+b^2}y + \left\{\omega^2\left(\frac{a^2-b^2}{a^2+b^2}\right)^2 - \omega'^2\right\}y = 0,$$
$$\frac{1}{\rho}\frac{\partial p}{\partial z} + Cz = 0.$$

Therefore, integrating,

$$\frac{p}{\rho} + \tfrac{1}{2}(Ax^2 + By^2 + Cz^2) - \omega(\omega + \omega')\frac{a^2 - b^2}{a^2 + b^2}(x^2 - y^2)$$

$$+ \tfrac{1}{2}\left\{\omega^2\left(\frac{a^2 - b^2}{a^2 + b^2}\right)^2 - \omega'^2\right\}(x^2 + y^2) = H,$$

a constant.

The surfaces of equal pressure can therefore be made similar to the external surface, and a free surface is therefore possible, if

$$a^2\left\{A - 2\omega(\omega + \omega')\frac{a^2 - b^2}{a^2 + b^2} + \omega^2\left(\frac{a^2 - b^2}{a^2 + b^2}\right)^2 - \omega'^2\right\}$$

$$= b^2\left\{B + 2\omega(\omega + \omega')\frac{a^2 - b^2}{a^2 + b^2} + \omega^2\left(\frac{a^2 - b^2}{a^2 + b^2}\right)^2 - \omega'^2\right\}$$

$$= c^2 C.$$

(If $\omega = 0$, we have the case considered by Jacobi; if $\omega' = 0$, we have the case first considered.)

These conditions may be written

$$a^2 A - a^2\left(\omega' + \omega\frac{a^2 - b^2}{a^2 + b^2}\right)^2 - 4a^2b^2\frac{a^2 - b^2}{(a^2 + b^2)^2}\omega^2$$

$$= b^2 B - b^2\left(\omega' - \omega\frac{a^2 - b^2}{a^2 + b^2}\right)^2 + 4a^2b^2\frac{a^2 - b^2}{(a^2 + b^2)^2}\omega^2$$

$$= c^2 C,$$

or

$$a^2 A - b^2 B$$

$$= 2\omega(\omega + \omega')(a^2 - b^2)$$

$$-\left\{\omega^2\left(\frac{a^2 - b^2}{a^2 + b^2}\right)^2 - \omega'^2\right\}(a^2 - b^2);$$

therefore

$$2\omega(\omega + \omega') - \omega^2\left(\frac{a^2 - b^2}{a^2 + b^2}\right)^2 + \omega'^2 = \frac{a^2 A - b^2 B}{a^2 - b^2},$$

or

$$(\omega + \omega')^2 + \frac{4a^2b^2}{(a^2 + b^2)^2}\omega'^2 = \int_0^\infty \frac{d\lambda}{(a^2 + \lambda)^{\frac{3}{2}}(b^2 + \lambda)^{\frac{3}{2}}(c^2 + \lambda)^{\frac{1}{2}}}.$$

Hence $a^2 A > b^2 B$ or $c^2 C$, and the greatest axis can never be the axis of revolution.

The axis of revolution is the mean axis or the least axis, according as

$$\left(\omega' - \omega\frac{a^2-b^2}{a^2+b^2}\right)^2 \lessgtr 4a^2\frac{a^2-b^2}{(a^2+b^2)^2}\omega^2.$$

If we put $\omega + \omega' = 0$, the ellipsoid is stationary, and we must have

$$a^2\left\{A - \frac{4a^2b^2}{(a^2+b^2)^2}\omega^2\right\} = b^2\left\{B - \frac{4a^2b^2}{(a^2+b^2)^2}\omega^2\right\} = c^2C;$$

the axis of revolution being then the least axis.

Again we may have a prolate spheroid, rotating in equilibrium about an equatorial axis, if

$$\left(\omega' - \omega\frac{a^2-b^2}{a^2+b^2}\right)^2 = 4a^2\frac{a^2-b^2}{(a^2+b^2)^2}\omega^2,$$

or

$$\frac{\omega'}{\omega} = \frac{a^2-b^2}{a^2+b^2}\left(1 \pm \frac{2a}{\sqrt{a^2-b^2}}\right).$$

## MONDAY, *May* 5, 1879.

### PROFESSOR G. D. LIVEING, PRESIDENT, IN THE CHAIR.

Mr WILLIAM HILLHOUSE, of Trinity College, was ballotted for and duly elected an Associate of the Society.

The following communication was made to the Society:—

PROFESSOR T. Mc K. HUGHES, *On the relation of the appearance and duration of the various forms of life upon the earth to the breaks in the continuity of the sedimentary strata.*

FEW subjects are more interesting than that of the first appearance of life upon the earth and the enquiry into the circumstances which have affected the duration of its various forms.

There are many difficulties in the way arising from the imperfect record we have in the rocks of any one district and the small knowledge of the surface of the earth which has yet been

gained, so that we cannot tell how far one district supplements another.

If a continental area with its various rocks were to go down and the sea to cover with sediment the irregular surface, filling up hollows and creeping up hills, and if we could after all this has happened get a clean cut through this new continuously-deposited sediment to the old rocks on which it was laid, though the material might be of the same kind as that which made up the older rocks, we should find evidence that the older series had been upheaved, had suffered denudation and gone down again before the newer series had begun to accumulate over it, and we should see that a long time must have elapsed between the formation of those old rocks and the earliest part of the newer deposit, that there had been an interruption in the geographical conditions, and we should say there was a *break* between the two series.

On the other hand, if we examined the newer sediment itself we should find that although it was made up of various material, here a pebble-beach, there a mudbank, in one part a coral reef, in another a heap of shells, still that it all was formed continuously during a period of depression, i.e. that there was *no break* in the series. That is to say, difference of lithological character does not involve lapse of time as does an unconformity, and so when we are considering the changes in the forms of life, between two dissimilar rocks, Cretaceous and Eocene for example, we must remember that there is not in this difference any evidence of a break in time, such as we find between Silurian and Upper Old Red, or between Carboniferous and Mercian, but the conditions which gave rise to the formation of chalk with its myriads of microscopic marine organisms were very different from those which allowed the accumulation of the estuarian and fluviatile beds of the Lower Eocene, such changes in sediment being the usual sudden effects of gradual operations such as the silting up of hollows, destruction of headlands, and such like.

A period or area of upheaval is essentially one of destruction, and the removed material is carried to the areas of depression for that period. So where we find in the rocks evidence of vast masses gone, we cannot *there* find traces of the life of the period, as there is there no sediment in which its remains could be preserved.

It is convenient to have a table of the known strata, and although we cannot arrange all the rocks of the world in parallel columns, and say that *ABC* of one area are exactly synchronous with *A′B′C′* of another, still if we take any one country and establish a grouping for it, we find so many horizons at which equivalent formations can be identified in distant places that we can generally make an approximation to homotaxis as Huxley called it. The most

convenient grouping is obviously to bracket together locally continuous deposits, i.e. all the sediment which was formed from the time when the land went down and accumulation began to the time when the sea bottom was raised and the work of destruction began.

In the accompanying table (Plate VI.) I have given the rocks of Great Britain classified on this system, and bearing in mind that waste in one place must be represented by deposit elsewhere, I have represented the periods of degradation by intervals estimated where possible by the amount of denudation known to have taken place between the periods of deposition in the same district.

It is obvious that when the dry land goes down there is an end of exclusively terrestrial life over that region, and when the sea bottom has been upheaved there can be no more exclusively marine forms over that area till it goes down again, and when the one comes up or the other goes down it will be invaded from adjoining areas by those forms of life for which it from time to time becomes adapted. But they may not be the same as those that inhabited it before.

Supposing then a submergence along the axis of the Mediterranean were to move south, so that Africa would by degrees sink, being always encroached upon by a deep sea creeping over it from the north, the land sinking on the south and rising on the north, so that Europe followed, extending on the north side of the sea, as Africa was then swallowed up on the south. First, we might imagine that the Alpine plants, which according to Hooker still linger in the high mountains of Morocco, would never cast their seed and grow from year to year so as to get across the equator and they would all perish. Whether the Black Sea fish and Caspian seals could get away round by France or would all disappear might be difficult to answer. When the tropical part of Africa was submerged its snakes, its lions, its elephants might hold their own till the Cape of Good Hope was reduced to an island too small for them.

But we have assumed that there would be land on the north side of this sea, and such forms as could migrate and adapt themselves to the climate would follow the receding sea. The monkeys from Gibraltar and from India would take the place of the gorilla and chimpanzee of the Tropics. The rhinoceros of Sumatra and the Asiatic elephant might replace their African cousins. The kite and the kestrel, the dolphin and the tunny, the lion and the tiger might still be there, but the ostrich and the giraffe would have no representatives.

How many genera, how many species would be common to the Old and New Africa, whether we searched its blown sand or its

fluviatile and lacustrine deposits? How many of the forms of life represented in the old upheaved bed of the Mediterranean on the north could be found in the waters and on the shore of the ocean in which the once midland sea was merged by the folds of earth's crumpling crust?

But the migration would not be necessarily, or even generally, *only* to the newly-submerged or newly-raised areas. An unsettling of the life stations in any area would cause those forms which could migrate to appear suddenly in adjoining areas where no movements were going on, so that their remains would appear in the middle of a continuous series of deposits. And the movement might not be from north to south across the equatorial region, so that many forms which could not endure extremes of heat or cold might travel on for ever round the earth if the movements did not necessitate their crossing unsuitable climes.

The short sketch I have just given is a fair sample of what has been going on over and over again on various parts of the earth's surface. If then we can read in the rocks the evidence of such succession of events as gave us many times sea where there had been land, and land where there had been sea, and we can find traces of the successive forms of life, it will be interesting to enquire what is the relation between the appearance upon the earth of distinct forms of life and the great changes in the physical geography of the areas over which they are found.

I can only gather a few examples here and there, but I think it will be seen that it is a line of enquiry for which a vast quantity of evidence is being rapidly accumulated.

Taking the oldest rocks of which we know anything in Britain, I refer you for a moment to that ancient series I brought before your notice on a former occasion, when I had just worked out their relations near Bangor and Carnarvon. These are the Pre-Cambrian rocks, perhaps the equivalents of the Huronian of America, but for the purposes of our enquiry to-day I refer to them only to dismiss them as we have not got a trace of life in them. It is true that in America traces of fossils are found in beds probably far older than our Carnarvon and Bangor beds, but these not universally admitted to be of organic origin; and in Britain we have nothing of the kind.

These ancient deposits were crumpled, raised above water, went down again, and on the irregular submerged land the Cambrian rocks were laid with here and there a shingle beach, and here and there sand and mud.

Referring to the table of strata, you will see that I take together under the old name Cambrian, under which they were first described, all the rocks from the conglomerates at the base of the Llanberis and Harlech groups to the top of the Bala Beds.

There were local checks in the movements as might be expected, especially in such a long time, but, on the whole, conditions were very similar over our area, and indeed very far beyond it during that enormous period of depression. The Silurian has at its base a stronger break than occurs between any of the subordinate divisions of the Cambrian Period, but still there is no great interval made out between the Silurian and Cambrian and they might with advantage be bracketed together, and take in also some beds which have passed as Old Red, and be all called by one of the old names, Transition Rocks or Greywacke Group.

Then comes a great interruption and waste of lands before the submergence which gave us the Upper Old Red conglomerates, the Devonian Rocks, and the Carboniferous, all, I take it, to be bracketed together as belonging to one set of conditions.

Then another great interval, and up and down over an enormous area. As we get on in the world's history, in Carboniferous times to some extent, but more in Mercian* times, and still more in the Anglian series, we find the deposits indicating distinct hydrographical areas, and this must influence the distribution of life.

Now with regard to the first appearance of life in Britain, there are some very curious facts to be noticed. In the earliest fossiliferous rocks, we have not anything like a common rudimentary form; but a large number of different families are represented, and represented by many genera and species.

They have turned up at different horizons, some low down, some higher up, but not in such a way as to suggest any grouping or order of succession, but rather to make it certain that it is only from our not having been able to find the remains preserved, that we do not get them abundantly all through, for they must have existed throughout the period.

Not at the base of the Harlech group, but where red slates come in, showing a local difference in the character of the sediment, we find a lingulella and several trilobites. Others come in at different horizons all the way up. Take for example the Trilobites Conocoryphe, Plutonia, Paradoxides, which occur down in the lowest beds. They appear, with many other forms, fully specialized and well developed. Just let us follow these up. Plutonia disappears at once; Paradoxides has its representative species in the Menevian, the next overlying group, and the genus then disappears;

* This term Mercian I use for all the deposits from the Lower New Red or Permian up to the top of the Jurassic Series, leaving in doubt for the present whether some of the estuarian and freshwater beds which show a silting up of the basin and emergence at the close of the period should be bracketed with the Jurassic or form the base of a new series. Under the name Anglian I include Neocomian and Cretaceous, as the term Cretaceous has become somewhat unsettled. At any rate we are safe in commencing a distinct group with the Lower Greensand, and not attaching great importance to the break which seems probably to occur in some places at the base of the Upper Greensand.

Conocoryphe has representative species in the Menevian, and allied forms appear in the Lingula Flags, Tremadoc and even Lower Bala Beds. Though it may be that the varieties with the more pronounced glabella, and indeed all the later forms may be separated from the typical genus, for our enquiry the name matters not. All allow that they are allied forms.

We might have taken Microdiscus and Agnostus instead of Paradoxides and Conocoryphe with similar result. Now except perhaps at the base of the Arenig, no one holds that there is any important break in the succession of strata over the area from which these forms have been procured. They do not appear immediately after an unconformity; they do not disappear just before one.

It is important to dwell upon the groups which appear in the earliest rocks yet discovered in Britain, for we shall see that so many forms of life are represented, and they range through subsequent periods to such varying lengths of time that there is nothing to suggest a different state of temperature, atmosphere, or other circumstances, which our recent experience tells us principally affect life.

The bivalve crustacea such as Leperditia are few and far between, and there is still less use calling in as evidence worm tracks which seem common to all periods or ill-understood fossils such as Oldhamia or the later Cruziana or obscure sponges. It is, however, very important to notice that in Theca we have the Pteropoda represented, and that although the small differences in that not very complex fossil have enabled palæontologists to assign different specific names to those which are found in almost every distinct horizon, and even to cut off Stenotheca and Cyrtotheca of the Menevian under different generic names, there is no unconformity between the horizons at which they occur and we follow the genus up into the upper beds of the Silurian without great variation of form; but of course free swimming oceanic creatures would be seldom affected by local changes.

Brachiopods appear among the first, being represented by the genera Discina, Obolella and Lingulella. The last which used to be called Lingula reaches its greatest numerical development in the next conformably succeeding group, named from the prevalence of this shell the Lingula Flags, and is represented by a closely allied form L. anatina, at the present time; so it at any rate has tided across a good many unconformities.

To move a little higher in the strata and watch new forms appearing. The Menevian is bracketed with the preceding group. There is no break between them; the *general facies* of the fossils is the same, some forms are identical. Yet here, and as far as discovery has gone, no lower, we get the trilobites, Arionellus, Anopolenus, Erinnys, Holocephalina.

Cystideans are found in this formation, and an addition to the brachiopoda in that most important Cambrian and Silurian genus Orthis. Now let us follow the fortunes of this genus as the world goes on.

The genus Orthis is first found a long way from the base of the Cambrian, only 4 or 5 species having been yet determined from beds older than the Lower Bala. The species and individual specimens are most numerous in the Bala group, and they begin to die out in the middle of the Silurian, so that by the time we get to the top of the Ludlow Rocks, we find only some varieties of O. elegantula. The interruption at the base of the May Hill Sandstone, whatever its character or amount, makes less difference in the appearance and disappearance of species of Orthis, than the long time in which there is no marked break from the Harlech to the Bala Beds, or from the base of the Silurian to the top of the same continuous series. We see that in the time of the Upper Ludlow deposits, the day of the Orthides was past, but they were not quite killed off by the great movements which took place before the deposition of the Carboniferous group. A few are found in the Devonian, and O. resupinata gets well up into the Carboniferous. Then they disappear. In the Bala and May Hill periods species of orthis come in, reach their maximum, and disappear, often characterizing very limited horizons, e.g. O. protensa, O. hirnantensis, O. sagittifera, O. spiriferoides, O. insularis, &c.

Other genera of brachiopods, such as Strophmena, yield very similar results.

To return to the table of strata. The number of new species which have been found in the Lingula Flags as compared with the Menevian has induced palæontologists to draw a strong boundary line between the two formations. Perhaps the most important new genera are the trilobites, Olenus and Dikellocephalus. Now it will be useful here to call attention to the remark of Salter that Conocoryphe, which we have seen was one of the earliest trilobites, was intermediate between the Oleni of the Middle Cambrian and the Calymenidæ of the Upper Cambrian and Silurian. Yet Olenus, of many species, and often very abundant in the Lingula Flags, has not been found below or with Conocoryphe, but is the characteristic fossil of a limited zone, coming in and going out in the midst of continuously and apparently somewhat uniformly deposited strata. The brachiopods of the Lingula Flags have had like most of the other fossils different specific names assigned to them from those in the underlying series.

We must notice the appearance of Niobe in the Lower Tremadoc preparing us for Ogygia and Asaphus in the Upper

Tremadoc, and itself disappearing at once. The same genera but different species of Pteropoda still prevail. So in the Upper Tremadoc the characteristic Angelina appears and is lost. With it we find Asaphus, Ogygia, Cheirurus, 2 genera and 3 species of Phyllopods, and Theca still among the Pteropods, but, in addition to Theca, we have now Bellerophon and Conularia, both of which genera last through long ages of Cambrian and Silurian, and, surviving the great geographical changes at their close, reappear in the Carboniferous. Conularia tides over another almost equally vast revolution, namely, that which preceded the New Red, and is last seen, not at the close of a period, but in the Lias, an early stage of the Jurassic epoch.

How long did it take to evolve the Cephalopoda with their cartilaginous cranium and optic ganglia? Hitherto we have found none in rocks older than the Upper Tremadoc. Salter, speaking of Cyrtoceras, remarks that many forms migrated in Cambrian times eastward from America, and are consequently of older date there than in Britain. But few, he adds, follow a reverse order of progression. Here, however, is the most hopeful line of enquiry, to seek in older rocks in other areas for the progenitor of the Tremadoc Orthoceras sericeum. The genus does not die out with the close of a period as far as evidence has yet been collected at home or abroad, for well down in the Carboniferous we lose it in Britain, and well up in the Hallstadt Beds we find it abroad.

In the next series we have disputed ground, some having bracketed the Arenig with the Lingula Flags and Tremadoc Beds, others having thought them, though continuous with the older rocks, so much more closely connected palæontologically with the overlying series that they have bracketed them with the Bala group, while some believe that there is an unconformity at the base of the Arenig. This opinion is partly founded on the large number of species found in the Arenig and not in the underlying series, but in this case it is more obvious than usual how valueless are percentages of species in common where there is not a fair representative series in each. In the Woodwardian Museum Catalogue (published 1873), there are 59 species recorded from the Arenig, and only 17 from the Upper Tremadoc; when that was drawn up it was clear we must have had 42 not common to both. In the newer Catalogue just published by the Museum of Practical Geology, there are 97 recorded from Arenig and only 23 from Upper Tremadoc, shewing 74 that must be peculiar to the Arenig.

However, after making allowance for this, there do seem to be a large number of new forms appearing for the first time in the Arenig beds. To begin, we have here the most characteristic group of Cambrian and Silurian fossils, the graptolites, 17 to 23

species are counted; all the most complex forms are here with single rows of cells, double rows, four rows back to back; graptolites branching once or many times, symmetrically or irregularly, all are represented*. Such a full complement of variously developed forms does certainly make one suspect that the group will be found in older beds, probably as low down as the base of the Cambrian at least, wherever suitable conditions prevailed.

The many branched forms soon disappear and the twin Graptolites do not get above the Lower Bala Beds. There is certainly no unconformity there. The Diprionidian forms survive the break at the top of the Cambrian, and die out *in the lowest beds* of the Silurian in Britain as in Bohemia. The apparent exception Retiolites which runs much higher belongs to a distinct group. Before we get to the top of the Silurian they have all completely gone.

We might take almost any of the genera or species of Arenig trilobites, and we should, in the same way tracing them on, find that some dropped out sooner and some later, but that they in no marked way ended their appearance at a recognised physical break, except perhaps the genus Trinucleus, which has not yet been shown to have got back into our area after the interval between the Cambrian and Silurian.

Lamellibranchs in the Arenig, represented by Palaearca and Ctenodonta, form a more conspicuous group in the Bala Beds, and increase in importance up to the top of the Silurian. For it must be noticed that although there may be a larger number of species of lamellibranchs in the Wenlock, the species of the Ludlow Rocks bear a much larger proportion to the rest of the life of that period.

Another group must be noticed though not so suitable for our purpose. Corals are not common except in the limestones, which of course they have largely helped to form. So Corals come in with the Limestones of Llandeilo and Bala, and as at present arranged there is hardly a genus which does not cross the gap between Cambrian and Silurian, and turn up again in the Wenlock Limestone; but here as well as in the case of the Echinoderms, we must remember the richness of the Wenlock in other fossils and their wonderful state of preservation, and also the ease with which they can be obtained.

Cystideans appeared early though few and far between, from the Protocystites of the Menevian to the Echinosphaerites and Sphaeronites of the Bala. Encrinite stems occur sometimes

* There was a suspicion of an allied form in the older rocks in Dictyonema sociale, and Mr Clifton Ward believes that beds from which he has obtained graptolites in the Lake district are of Tremadoc age.

plentifully in the Bala Beds. But it was in the Wenlock of all the older rocks that the tribe of stone lilies flourished most, while the starfishes are most developed in the Ludlow. Encrinites appeared abundantly now and then in later times, often characterizing deposits of small extent horizontally and vertically; Woodocrinus, for instance, having been found only at Richmond in Yorkshire, and E. liliiformis being confined to the Muschelkalk. Representative forms exist at the present day. Changes of currents, of temperature, and of their floating food, &c. must have caused them to disappear from the areas where they were once so abundant, for though Encrinites were fixed they did not grow like a plant, and got no more nourishment from the soil where they flourished than an oyster from the outside of an old bottle on which we find them sometimes growing. Encrinites could not migrate, but as in the case of oysters their spat might.

Though several genera of Gasteropoda occur at various horizons in the Bala Beds, they are never sufficiently numerous to allow us to infer anything from their absence elsewhere.

I have followed Orthoceras as the earliest Cephalopod through its range, but if we take any other Cambrian or Silurian form, and notably Lituites, we shall see how common forms begin and end in the middle of uninterrupted deposits. Phragmoceras certainly begins with the Silurian, but seems to be the only genus of the Cephalopoda that does so.

Quite at the base of the Silurian (i. e. in the May Hill Sandstone including Lower and Upper Llandovery) a number of new forms are seen for the first time. All that marked group of brachiopodous shells, the Pentameri, and with them Stricklandinia, are here first strongly represented. They have not all been found down to the base of the group. Stricklandinia lens appears early. Pentamerus oblongus and P. globosus hardly occur below the upper division. But though they come in suddenly after a break they go out suddenly in the midst of continuous deposits, before we get fairly into the Wenlock. Another species, the Pentamerus Knightii, a large and well-marked form just appears in the middle of the Ludlow Rocks, being almost confined to the Aymestry Limestone. Of Meristella we may tell the same tale (just expressing a doubt as to Meristella angustifrons). It is a genus very characteristic of the base of the Silurian, dying out by species as we ascend. The genera Leptaena and Strophomena have a long range, but rarely either generally or specifically are their appearances coincident with any physical breaks.

I will not dwell much more on the details of the Silurian fossils, many of which I have already commented upon in tracing types up from the Cambrian, but I may remark in passing that the evidence we obtain from them is just the same.

New forms or striking modifications of old forms appear in the Wenlock which succeeds the May Hill Beds quite conformably, and again in the Ludlow, which passes so gradually into the Wenlock that in the absence of the Wenlock Limestone it is almost impossible to draw a line between them. When we get into the Limestone the immense abundance of corals, encrinites and cystideans, as before pointed out, though they swell the number of species, do not form fair ground of comparison with non-calcareous strata. Many new forms of Trilobites appear, Sphaerexochus, Acidaspis, Cyphaspis, &c., and whatever they be, there is no suggestion of any unconformity in this series, even carrying it over the great Lamellibranch zone at the top of the Ludlow, and far above the Silurian through the Ledbury shales into the Lower Old Red.

Let us notice what becomes of the Trilobites eventually. Several old genera, as Bronteus, Homalonotus, Phacops, Proetus, get into the Devonian, tiding over what is locally at any rate a great unconformity, and that is the last seen of those genera. All the family of Trilobites die out long before the close of the Carboniferous, in which Phillipsia and Griffithides represent that abundant family to which we had principally to refer in classifying the Cambrian and Silurian Rocks.

It is true that where the Trilobites die out the Limuli, represented long before by Neolimulus falcatus of the Wenlock, become more common, but there is a great gap between these two groups; and there was not in the last Trilobites any approach to the Limuloid type.

It seems that the evidence so far, making allowance for the imperfection of the record and the limited search which has been made in many areas, goes to show that whether we consider the smaller groups, as varieties and species, or the larger as genera, new forms appear at various horizons in *uninterrupted deposits*, and that they die out in the same way; and that after a long lapse of time, as measured by deposition, there is *caeteris paribus* as great a change in the life of the period as we find after a similar interval measured by denudation.

We speak of higher and lower forms of life. It is not meant that the higher is better fitted for its surroundings than the lower, but the term higher is applied to those forms which have a more complex arrangement of organs for discharging the varied functions of life. And there certainly seems to have been an increase of higher forms as time went on. So it is interesting to take note of the first appearance of some of these and test its bearing upon the question we are considering. In the Lower Ludlow, i.e. high up in the Silurian Rocks, we have the earliest yet known remains of fishes. Yet we ought to have found them had they been there,

for the head shield of Pteraspis, for instance, was a thing easily preserved and recognised.

Quite at the top of the Ludlow Rocks we have a bone bed full of remains of Onchus and Thelodus, which from their likeness to sharks we may suppose to have had a high brain organization. There is a considerable difference between this group and that which occurs in the Upper Old Red or base of the Carboniferous series, but a long time, measured by long deposition and enormous intermediate denudation, has elapsed between the two periods. When we find the Cambrian fish, which I fully expect we shall, we shall see that they too are very different from those of the Ludlow Rocks.

In the succeeding Devonian and Carboniferous periods they abound. But the doubts as to the grouping of the Old Red and Devonian, and the limited distribution and range of this group, renders it less suitable for our present purpose. As far as the evidence does go it quite confirms the inference we have already arrived at: that lapse of time, whether measured by deposition or denudation, generally is accompanied by the introduction of new or modified forms of life.

Whatever may be said of the extension of the classification here adopted, as far as I am inclined to apply it, it is clear that we may consider a great part of the Devonian as a basement series to the Carboniferous; and bearing this in mind we will take as an example the genus Producta, and trace it back to see whether it comes in after an unconformity. We do, it is true, find it low down in the Mountain limestone, even when that rests on the upturned edges of the Silurian, as in the Craven area in Yorkshire; but unless we recognise it as represented in the Productella of the Devonian or the Chonetes and Leptaena of the Silurian and Cambrian, it is not found in earlier beds.

When the Carboniferous sea basin, in which the coral and the encrinite grew and the Productidae thrived in thousands, was being silted up or raised, then the animals of the clear sea or their spawn migrated to less unfavourable areas, and appeared as new forms in the midst of locally continuous deposits. As to what were the forms from which they were originally modified we have only rarely a suggestion. For instance, the Cephalopoda are, in the Devonian, modified in the position of the siphuncle and other characters, so as to foreshadow the two great groups of the next period; the Goniatites, with its dorsal siphuncle, leading up to the Ammonites and the Clymenia to the genus Nautilus. What variation in character was produced in the process of acclimatisation in the new home of each part of their race we seldom can examine closely enough to enquire, but we may hope by and bye to get from this line of enquiry also some evidence bearing upon the great geographical changes our earth has undergone.

It seems perfectly clear that there are and always have been earth movements going on which have perpetually unsettled the condition affecting life, and that these movements are slow and more or less regular in their action.

The sequence of life upon the earth, showing a gradual incoming of new or modified forms all through, and not a succession of total extinctions and replenishments, points to the persistence of oceanic and continental areas, as an interruption in the continuity of suitable land and climate or of water of the required temperature, salinity or depth, would be destructive to life. Therefore the movement must have been of the nature of an earth-wave, the land rising on one side and sinking on the other of a given area.

Stratigraphical evidence shows that the submergence which allowed of the accumulation of sediment often commenced earlier over one part of the area than another, and that the direction of the movement of such troughs was not always the same. The foregoing palæontological considerations confirm this. Only some forms of life succeed in migrating because some directions are more fatal than others, namely, those which necessitate travelling across greater extremes of conditions affecting life.

It is only reasoning in a circle to define formations palæontologically and then to speak of the incoming and outgoing of species as nearly coincident with the beginning and end of the formation, but as denudation and deposition are necessarily equal, and no denudation can take place until the solid matter of the earth's crust has been lifted up within reach of the denuding agents, the periods of these earth-waves must somewhat coincide with the periods of deposition and denudation as given in the accompanying table, and these form our geological measures of time.

Whether the transference of the immense masses of denuded material always to the coastline of continents is the cause or effect, or modifies the direction of the earth-waves, is a question as yet unanswered.

## Monday, *May* 19, 1879.

Professor G. D. Liveing, President, in the Chair.

The following communications were made to the Society:—

(1) Professor Liveing, *On the dispersion of a solution of mercuric iodide.*

In the *Chemical News*, vol. 29, p. 128, Mr E. Sonstadt proposes the use of a solution of mercuric iodide combined with potassium

iodide as a convenient liquid for testing specific gravities, and mentions that he has obtained it with a specific gravity upwards of three times that of water. Being in search of a substance which would give a considerable dispersion of the red end of the spectrum I thought I would try the dispersive power of this solution; and though its power of dispersing the red is not nearly so great as its power of dispersing the green, its dispersive power is still very remarkable throughout the range of colours which it transmits. My assistant, Mr Robinson, has prepared the solution for me, but though he has obtained it of sp. gr. 3·013 at a temperature of 17° C. since the observations described below were made, the liquid of which I have examined the dispersion is of sp. gr. 2·77 at 18° C. It is of a pale yellow colour, and seems to absorb completely the violet and indigo light. On observing the solar spectrum through it there is no perceptible diminution of light until nearly half way between $F$ and $G$; the absorption begins there and thence increases very rapidly. At the red end $A$ could be seen, so there is not much absorption of visible rays of low refrangibility.

Filling a hollow prism of 59° 11′ with the liquid, I obtained the following indices of refraction:

| *A* | *B* | *C* | *D* | *E* | *F* |
|---|---|---|---|---|---|
| 1·628 | 1·637 | 1·641 | 1·654 | 1·673 | 1·693 |

Taking $E$ as the standard ray, I get for the dispersive power $\left(\frac{\mu_2-\mu_1}{\mu_0-1}\right)$ between

| *A—B* | *B—D* | *D—E* | *E—F* |
|---|---|---|---|
| ·01263 | ·02555 | ·02822 | ·02896 |

Comparing these figures with those given in Watts' Dictionary for dense flint-glass and for carbon disulphide, we have refractive indices

| | *A* | *B* | *C* | *D* | *E* | *F* |
|---|---|---|---|---|---|---|
| Flint-glass | | 1·6278 | 1·6297 | 1·6350 | 1·6420 | 1·6483 |
| $CS^2$ at 11° | 1·6142 | 1·6207 | | 1·6333 | 1·6465 | 1·6584 |

giving dispersive powers

| | *A—B* | *B—D* | *D—E* | *E—F* |
|---|---|---|---|---|
| Flint-glass | | ·01134 | ·01089 | ·00972 |
| $CS^2$ | ·01005 | ·01949 | ·02042 | ·01841 |

The dispersive power of the mercury solution is therefore very nearly three times that of flint-glass for green light and more than double for orange light: and it is more than one-half greater than that of carbon disulphide for green light, and nearly one-third

greater for orange light. So the green part of the spectrum is in comparison with the red even more drawn out by the mercury solution than by carbon disulphide. The figures above given depend only on one set of observations, and are therefore open to correction by future examination.

(2) Professor LIVEING, *On a new spectroscope.*

The aim of this instrument is to obtain considerable dispersion with as little loss of light as possible, and at the same time good definition in all parts of the spectrum. Young has pointed out the economy of using one half-prism attached to the collimator with one face perpendicular to the axis, and another similar half-prism attached in a similar way to the telescope. A beam of light can thus be used larger, in proportion to the size of the face of the prism, than when a single prism with an angle equal to the sum of the angles of the half-prisms is used. Thollon has given a mathematical investigation to show that when two half-prisms are thus used and the incident and emergent light is normal to the first and last faces of the pair respectively, the deviation is a minimum. It is, however, easy to see without analysis that two prisms of 30° placed as in Fig. 1 (Plate VII.), so as to be equivalent to a single prism of 60° at minimum deviation, have the angles of incidence and refraction the same as when they are placed as in Fig. 2, only these angles occur in a different order. The deviation will be the same in each case, and there will be equal clearness of definition. In the spectroscope now described, two half-prisms are attached to the collimator and telescope respectively, and there are two whole prisms between them. These however are all compound prisms, so as to get considerable dispersion without the loss of light involved in a very oblique incidence.

With so great a dispersion as these two whole and two half-prisms give, it is necessary to adjust the prisms to minimum deviation, or nearly so, in order to get good definition, or even to see faint lines at the extremities of the spectrum. In order, therefore, to be able to use it for the kind of work in which the author has, in conjunction with Professor Dewar, been for some time past engaged, which often involves a rapid transition from one end of the spectrum to the other, some automatic adjustment of the prisms was desirable. This is effected by a simple system which cannot easily go wrong. The collimator *L*, see Fig. 3, is fixed to the table of the instrument so that the prolongation of the first face of the half-prism attached to it may pass through the centre of the table; a broad arm *E*, turning on an axis *C* at the centre of the table, carries the telescope *T* and a vernier by which the angle through which the telescope is turned can be read upon the graduated rim of the table. The two whole prisms are each carried

by an arm moveable about the same axis $C$ as the telescope. The plane through the edge of the first prism $A$ and the axis $C$ is made always to bisect the angle between the plane through the edge of $B$ and the same axis and the plane which is the prolongation of the first face of the half-prism attached to the collimator. This is effected by two levers, one working on a pivot $e$ fixed to the table, and the other working on a pivot $g$ fixed to the arm $F$ which carries the prism $B$. These two levers have at their other ends a common pivot $f$, which works in a slot in the arm $G$ which carries the prism $A$. The line $CA$ thus always bisects the angle $gCe$. By similar levers attached to the arms $E$ and $G$ the line $BC$ is made always to bisect the angle $lCh$. By this mechanism the prisms are always kept symmetrically arranged. Measurements are taken by moving the telescope until the line to be measured is on the cross wires of the telescope. It is not of any consequence in practice whether the prisms be always exactly at the minimum deviation, provided they be nearly so, but it is important, where measures are taken by readings of the angle through which the telescope is moved, that the readings should always be the same when the same line is on the cross wires. To ensure this the author purposes to have spiral springs attached to the several moveable arms, so as to keep them always up to their bearings on one side. The instrument has been made by Browning. Were another such instrument constructed it might perhaps be well to replace the flat arm with a slot which may wear to uneven edges, by an arm with its middle part square in section and a tube sliding on it to which the levers should be hinged as in Fig. 4. The wear in this case would be more uniform and of less consequence.

To illuminate the cross-wires when the field is dark the author proposes to place a small collimator, with a pin-hole instead of a slit, so that the light from it may be reflected from the second face of the second prism. Since the angle through which this prism moves is always half that through which the telescope is moved the image of the pin-hole will be in a nearly constant position in the field of view; and it may be conveniently illuminated by a sodium flame.

(3) Mr C. Taylor, M.A., *On the geometrical proof of Lambert's theorem.*

Lambert's theorem on the times of describing portions of an elliptic orbit was proved in the fourth section of his work on comets entitled *Insigniores Orbitæ Cometarum Proprietates* (1761).

The author's demonstration although essentially geometrical was encumbered by calculations and reductions which may be seen

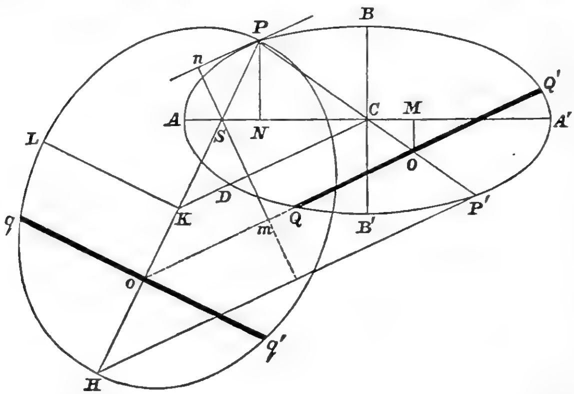
to be unnecessary, for the theorem may be proved by a purely geometrical process which presents no serious difficulty.

We have to prove that:

*The time in an arc of an elliptic orbit described about a focus may be expressed in terms of the major axis, the chord of the arc, and the sum of the distances from that focus to the extremities of the arc.*

## I.

Let $S$ be a focus of an ellipse, and $AA'$ and $BB'$ its axes.




Take any fixed diameter $PCP'$ and let $CD$ be the conjugate semi-diameter. Let $PS$ meet $CD$ in $K$ and the tangent at $P'$ in $H$. Then $PK$ is equal to $CA$ and $PH$ to $AA'$.

Also, if $CN$ be the abscissa of $P$,

$$SK : CN = CS : CA,$$

or briefly $$SK = e\,.\,CN,$$

if $e$ be used to denote the eccentricity.

With $S$ as focus and $P$ and $H$ as vertices describe a second ellipse, and let $KL$ be its semi-axis conjugate; then, if $S'$ be the further focus of the first ellipse,

$$CD^2 = SP\,.\,PS' = SP\,.\,SH = KL^2.$$

In the second ellipse place any principal double ordinate $qq'$ bisected in $o$; and in the first ellipse let $QOQ'$ be that double ordinate of $PP'$ which passes through $o$. Then we have to shew:

(i) That $QQ' = qq'$.

(ii) That $SQ + SQ' = Sq + Sq'$.

(iii) That the areas of the sectors $SQQ'$ and $Sqq'$ are in the subduplicate ratio of the latera recta of their ellipses.

(i) By the property of ordinates and by parallels,

$$qo^2 : KL^2 = oH \,.\, oP : KP^2 = OP \,.\, OP' : CP^2$$
$$= QO^2 : CD^2;$$

and since $KL$ is equal to $CD$, therefore $QO$ and $qo$ are equal, or

$$QQ' = qq'.$$

(ii) From above

$$SK : CN = CS : CA = CS : PK.$$

And by parallels, if $CM$ be the abscissa of $O$ the middle point of $QQ'$,

$$CN : CM = CP : CO = PK : Ko.$$

Therefore $$SK : CM = CS : Ko;$$

or in terms of the eccentricities $e$ and $e'$,

$$e \,.\, CM = e' \,.\, Ko.$$

Hence

$$SQ + SQ' = AA' + 2e \,.\, CM = PH + 2e' \,.\, Ko$$
$$= Sq + Sq'.$$

(iii) The elements of area which the equal ordinates $QQ'$ and $qq'$ in any two consecutive positions cut off from their ellipses are as the *breadths* of those elements.

Let a chord through $S$ meet $QQ'$ and the tangent at $P$ at right angles in $m$ and $n$, and let the breadths of the elements be estimated on $Sm$ and $So$ respectively; thus it appears that the elements are in the *constant ratio* of $Sm$ to $So$, and hence that the whole segments $QP'Q'$ and $qHq'$ are in that ratio.

But this is also the ratio of the triangles $SQQ'$ and $Sqq'$ on equal bases $QQ'$ and $qq'$.

Therefore *the elliptic sectors $SQQ'$ and $Sqq'$ are* as $Sm$ to $So$, or as $Sn$ to $SP$, or as $CB$ to $CD$; that is to say, they are in the ratio of the minor axes, and therefore ($PH$ being equal to $AA'$) *in the subduplicate ratio of the latera recta of their ellipses.*

It follows that the arcs $QP'Q'$ and $qHq'$ are described in *equal times;* or in other words, that the time in the arc $QP'Q'$ depends only upon the lengths $AA'$, $QQ'$, and $SQ + SQ'$, as was to be proved*.

At the outset we supposed one of the two isochronous arcs to

* It needs some consideration to see that the theorem is now proved; but by following out the line of argument briefly indicated in the next paragraph it will appear that the proof is in reality quite general.

be cut off by a chord $qq'$ *at right angles* to the axis; but the restriction may be removed by supposing the first ellipse to change its form whilst the form and dimensions of the second remain (for the time being) invariable, and by comparing the several forms of the first ellipse with the second, and thus with one another.

Take a length $SX$ equal to $AA'$, and on $SX$ take a length $RR'$ equal to $QQ'$, and such that $SR + SR' = SQ + SQ'$. Then, $SX$ being regarded as a flat ellipse*, the time in the arc $QQ'$ is equal to the time in which a comet falling into the sun $S$ from rest at $X$ would traverse the distance $RR'$†.

The above, which is a simplification of Lambert's own proof, applies to the Hyperbola as well as to the Ellipse‡.

The case of the Parabola is treated§ separately in *Sectio* II. theor. 4, where it is remarked by anticipation: "Insignis hæc motus cometarum parabolici proprietas, si debite limitetur, ceteris quoque Sectionibus conicis adplicabilis est" (p. 45). In the last page of the Preface he speaks expressly of the Hyperbola.

## II.

The construction might also have been made as follows:

Draw two separate ellipses on equal major axes, and take equal focal radii $SP$ in the one and $sp$ in the other. Let any pair of chords $QQ'$ and $qq'$ which make equal intercepts $PE$ and $pe$ on $SP$ and $sp$ be ordinately applied to the diameters through

* The line $SPX$ equal to $AA'$ is a limiting form of the first ellipse, whose further focus may be at any point on the circle drawn with $P$ as centre and radius equal to $AA' - SP$.

† Otherwise thus: write down the expression for the time in the arc $qq'$ of the auxiliary ellipse, and deduce the expression for the time in $QQ'$ in terms of $AA'$, $QQ'$, and $SQ + SQ'$.

‡ I find that Lexell gave the proof of the property in question geometrically (making however some slight use of trigonometrical ratios) in 1784; but the complete proof as he gave it is so elaborated (*Nova Acta Academiæ Scientiarum Imperialis Petropolitanæ*, tom I. pp. (141)—(146), (149), (150), 1787), that it makes the theorem appear less simple geometrically than it really is. He failed to appreciate the generality of Lambert's proof through not observing that the first ellipse was to be regarded as variable in relation to the second, and he was under the impression that he had extended the theorem by remarking that it was applicable to the hyperbola. See pp. (141), (149). For the reference to Lexell I am indebted to Chasles, *Aperçu Historique*, p. 187 (ed. 2, 1875), where it is said: "La propriété de l'ellipse qui est le fondement de ce théorème convient aussi aux secteurs de l'hyperbole, ainsi que l'a démontré par de simples considérations de Géométrie le célèbre Lexell."

§ Euler expressed the area of a parabolic sector $ASP$ in terms of $\tan \frac{1}{2} ASP$, and thus determined the time. See his *Theoria motuum Planetarum et Cometarum*, Prob. IV. p. 16 and Prob. VII. p. 29, to which work Lambert refers at the end of his Preface. I learn from Professor Adams that Euler also gave the equation commonly referred to as Lambert's in the case of the Parabola, viz. in the *Miscellanea Berolinensia*, Tomus VII. pp. 19, 20 (1743).

$P$ and $p$ respectively; then it may be shewn as before that the chords are equal and subtend isochronous arcs, and that $SQ + SQ'$ is equal to $sq + sq'$.

With this latter construction it follows from Newton's *Principia* (Lib. I. Sect. II. prop. 6, theor. 5) as an immediate consequence of the equality of the intercepts $PE$ and $pe$ that (the forces at $P$ and $p$ being equal) the arcs $QQ'$ and $qq'$, supposed *infinitesimal*, are described in equal times: Lambert shews that these arcs are still isochronous when the equal intercepts are *finite*.

## III.

Another proof for the Ellipse by orthogonal projection is implicitly contained in *Sectio* IV. §§ 180, 181, 186.

*a.* Through a variable point $E$ on $SP$ draw a double ordinate $QQ'$ (bisected in $O$) to the diameter through $P$, and let the ellipse be projected on to its auxiliary circle, and let small letters be now used to denote the points in the circle corresponding to the above. Then, since

$$PE : PK = PO : PC = po : pC,$$

(the point $K$ being determined as in the first proof,) it follows that *the diametral sagitta $po$ of the circular arc $qq'$ is equal to $PE$.*

By making $E$ coincide with $S$ we deduce that *the altitude of the triangle $Sqq'$ is equal to $SE$.*

*b.* Hence, if we take two ellipses having equal auxiliary circles, as in the construction II., and take $SP$ and $PE$ in the one and equal corresponding lengths in the other, it follows (1) that $po$ is the same for both, and therefore that the isochronous arcs of the ellipses correspond to *equal* arcs of their auxiliary circles; and (2) that the altitude of the triangle $Sqq'$ is the same for both.

Hence the area of the sector $Sqq'$ is the same in both circles, and the corresponding areas in the ellipses are therefore in the subduplicate ratio of their latera recta*.

* We can at once write down an expression for the circular area $Sqq'$ in terms of the segments of the line $PESK$, viz. from the relation

$$\text{sector } Sqq' = \text{sector } Cqq' - \Delta\,(Cqq' - Sqq').$$

Let the semi-axes of the ellipse be $a$ and $b$, and let $PE = x = po$, and $SP = r$. Then

$$\text{sector } Sqq' = a^2 \operatorname{vers}^{-1}\frac{x}{a} - (a-r)\sqrt{(2ax - x^2)}.$$

The elliptic sector $SQQ'$ is therefore equal to

$$\frac{b}{a}\left\{a^2 \operatorname{vers}^{-1}\frac{x}{a} - (a-r)\sqrt{(2ax - x^2)}\right\}.$$

*c.* If $Co$ meet the focal chord parallel to $qq'$ in $m$, and if $CM$ be the abscissa of $O$,

$$CS \,.\, CM = Cm \,.\, Co = SK \,.\, EK,$$

which is the same for both ellipses. Hence $SQ + SQ'$ is the same for both ellipses.

*d.* To determine pairs of isochronous arcs in the two ellipses, find two equal diameters and draw the corresponding diameters of their auxiliary circles, and draw equal chords in the circles parallel to those diameters: then will the projections of those chords upon the ellipses determine isochronous arcs as required.

IV.

The expression given by Lambert (after Euler) for the area of a focal sector $SMN$ of a parabola may be obtained as follows.

If $P$ be the vertex of the diameter bisecting the chord $MN$, viz. in $V$, then $SM + SN = 2(SP + PV)$ and $MN = 4\sqrt{SP \,.\, PV}$; and therefore

$$\frac{SM + SN \pm MN}{2} = (\sqrt{SP} \pm \sqrt{PV})^2.$$

Hence

$$\left\{\frac{SM + SN + MN}{2}\right\}^{\frac{3}{2}} - \left\{\frac{SM + SN - MN}{2}\right\}^{\frac{3}{2}}$$
$$= (\sqrt{SP} + \sqrt{PV})^3 - (\sqrt{SP} - \sqrt{PV})^3$$
$$= \sqrt{PV}\,(6SP + 2PV)$$
$$= \frac{3}{\sqrt{AS}} \text{ sector } SMN,$$

as is otherwise proved by Lambert in *Sectio* I. §§ 60—63.

(4) Mr F. M. BALFOUR, M.A., F.R.S., *On certain points in the anatomy of Peripatus Capensis.*

The discovery by Mr Moseley* of a tracheal system in Peripatus must be reckoned as one of the most interesting results obtained by the naturalists of the *Challenger*. The discovery clearly proves that the genus Peripatus, which is widely distributed over the globe, is the persisting remnant of what was probably a large group of forms, from which the present tracheate Arthropoda are descended.

The affinities of Peripatus render any further light on its

* "On the structure and development of Peripatus Capensis," *Phil. Trans.*, Vol. CLXIV., 1874.

anatomy a matter of some interest; and through the kindness of Mr Moseley I have had an opportunity of making investigations on some well-preserved examples of Peripatus Capensis, a few of the results of which I propose to lay before the Society.

I shall confine my observations to three organs. (1) The segmental organs, (2) the nervous system, (3) the so-called fat bodies of Mr Moseley.

In all the segments of the body, with the exception of the first two or three postoral ones, there are present glandular bodies apparently equivalent to the segmental organs of Annelids.

These organs have not completely escaped the attention of previous observers. The anterior of them were noticed by Grube*, but their relations were not made out. By Saenger†, as I gather from Leuckart's *Bericht* for the years 1868-9, these structures were also noticed, and they were interpreted as segmental organs. Their external openings were correctly identified. They are not mentioned by Moseley, and no notice of them is to be found in the text-books. The observations of Grube and Saenger seem, in fact, to have been completely forgotten.

The organs are placed at the bases of the feet in two lateral divisions of the body-cavity shut off from the main median division of the body-cavity by longitudinal septa of transverse muscles.

Each fully developed organ consists of three parts:

(1) A dilated vesicle opening externally at the base of a foot.

(2) A coiled glandular tube connected with this, and subdivided again into several minor divisions.

(3) A short terminal portion opening at one extremity into the coiled tube (2) and at the other, as I believe, into the body-cavity. This section becomes very conspicuous in stained preparations by the intensity with which the nuclei of its walls absorb the colouring matter.

The segmental organs of Peripatus, though formed on a type of their own, more nearly resemble those of the Leech than of any other form with which I am acquainted. The annelidan affinities shown by their presence are of some interest. Around the segmental organs in the feet are peculiar cells richly supplied with tracheæ, which appear to me to be similar to the fat bodies in insects. There are two glandular bodies in the feet in addition to the segmental organs.

The more obvious features of the nervous system have been fully made out by previous observers, who have shown that it consists of large paired supraœsophageal ganglia connected with

* "Bau von Perip. Edwardii," *Archiv f. Anat. u. Phys.*, 1853.

† *Moskauer Naturforscher Sammlung*, Abth. Zool., 1869.

two widely separated ventral cords,—stated by them not to be ganglionated. Grube describes the two cords as falling into one another behind the anus—a feature the presence of which is erroneously denied by Saenger. The lateral cords are united by numerous (5 or 6 for each segment) transverse cords.

The nervous system would appear at first sight to be very lowly organized, but the new points I believe myself to have made out, as well as certain previously known features in it, appear to me to show that this is not the case.

The following is a summary of the fresh points I have observed in the nervous system:

(1) Immediately underneath the œsophagus the œsophageal commissures dilate and form a pair of ganglia equivalent to the annelidan and arthropodan subœsophageal ganglia. These ganglia are closely approximated and united by 5 or 6 commissures. They give off large nerves to the oral papillæ.

(2) The ventral nerve cords are covered on their ventral side by a thick ganglionic layer*, and at each pair of feet they dilate into a small but distinct *ganglionic swelling*. From each ganglionic swelling are given off a pair of large nerves† to the feet; and the ganglionic swellings of the two cords are connected together by a pair of commissures containing ganglion cells‡. The other commissures connecting the two cords together do not contain ganglion cells.

The chief feature in which Peripatus was supposed to differ from normal Arthropoda and Annelida, viz. the absence of ganglia on the ventral commissures, does not really exist. In other particulars, as in the amount of nerve cells in the ventral cords and the completeness of the commissural connection between the two cords, etc., the organization of the nervous system of Peripatus ranks distinctly high. The nervous system lies within the circular and longitudinal muscles, and is thus not in proximity with the skin. In this respect also Peripatus shews no signs of a primitive condition of the nervous system. A median nerve is given off from the posterior border of the supraœsophageal ganglion to the œsophagus which probably forms a rudimentary sympathetic system. I believe also that I have found traces of a paired sympathetic system.

The organ doubtfully spoken of by Mr Moseley as a fat body, and by Grube as a lateral canal, is in reality a glandular tube, lined by beautiful columnar cells, which opens by means of a non-glandular duct into the mouth. It lies close above the ventral nerve

* This was known to Grube, *loc. cit.*

† These nerves were noticed by Milne Edwards, but Grube failed to observe that they were much larger than the nerves given off between the feet.

‡ These commissures were perhaps observed by Saenger (*loc. cit.*).

cords in a lateral compartment of the body-cavity, and extends backwards for a varying distance.

This organ may perhaps be best compared with the simple salivary-gland of Julus. It is not to be confused with the slime-glands of Mr Moseley which have their opening in the oral papillæ. If I am correct in regarding it as homologous with the salivary glands so widely distributed amongst the Tracheata, its presence indicates a hitherto unnoticed arthropodan affinity in Peripatus.

(5) Mr J. W. L. GLAISHER, M.A., F.R.S., *On a symbolic theorem involving repeated differentiations.*

The theorem is

$$\left(\frac{d^2}{dx^2}-a^2\right)^n \frac{e^{ax}}{x} = (-)^n\, 2^n . n!\left(\frac{1}{x}\frac{d}{dx}\right)^n \frac{e^{ax}}{x} \text{ ..................(1).}$$

It can be established by direct expansion, for it is not difficult to verify that each side of the equation is equal to

$$(-)^n \frac{(2a)^n}{x^{n+1}}\, n!\, \left\{1 - \frac{n+1\,.\,n}{2}\frac{1}{ax} + \frac{n+2\,.\,n+1\,.\,n\,.\,n-1}{2\,.\,4}\frac{1}{a^2x^2} - \&\text{c.}\right\} \frac{e^{ax}}{x},$$

but it may also be proved by means of the integral

$$\int_0^\infty e^{-a^2x^2-\frac{b^2}{x^2}}\, dx = \frac{\sqrt{\pi}}{2a}\, e^{-2ab}$$

as follows:

Let $u$ denote this integral; then

$$\frac{du}{da} = \int_0^\infty - 2ax^2 e^{-a^2x^2-\frac{b^2}{x^2}}\, dx,$$

so that

$$\left(-\frac{1}{2a}\frac{d}{da}\right) u = \int_0^\infty x^2 e^{-a^2x^2-\frac{b^2}{x^2}}\, dx,$$

and therefore

$$\left(-\frac{1}{2a}\frac{d}{da}\right)^n u = \int_0^\infty x^{2n}\, e^{-a^2x^2-\frac{b^2}{x^2}}\, dx \text{ ...............(2).}$$

Again

$$\frac{d^2u}{da^2} = \int_0^\infty (-2x^2 + 4a^2x^4)\, e^{-a^2x^2-\frac{b^2}{x^2}}\, dx,$$

and

$$\int_0^\infty 4a^2x^4 e^{-a^2x^2-\frac{b^2}{x^2}}\,dx = \left[-e^{-a^2x^2}2x^3 e^{-\frac{b^2}{x^2}}\right]_0^\infty$$

$$+\int_0^\infty e^{-a^2x^2}\frac{d}{dx}\left(2x^3 e^{-\frac{b^2}{x^2}}\right)dx = \int_0^\infty (6x^2+4b^2)\,e^{-a^2x^2-\frac{b^2}{x^2}}\,dx,$$

therefore

$$\frac{d^2u}{da^2} = \int_0^\infty (4x^2+4b^2)\,e^{-a^2x^2-\frac{b^2}{x^2}}\,dx,$$

whence

$$\left(\frac{d^2}{da^2}-4b^2\right)u = 4\int_0^\infty x^2 e^{-a^2x^2-\frac{b^2}{x^2}}\,dx \quad\text{.................}(3).$$

If instead of the integral $u$ we start with the integral

$$u_n = \int_0^\infty x^{2n} e^{-a^2x^2-\frac{b^2}{x^2}}\,dx$$

we have

$$\frac{d^2u_n}{da^2} = \int_0^\infty \left(-2x^{2n+2}+4a^2x^{2n+4}\right)e^{-a^2x^2-\frac{b^2}{x^2}}\,dx,$$

and integrating the second term by parts as before, we find

$$\frac{d^2u_n}{da^2} = \int_0^\infty \{(4n+4)\,x^{2n+2}+4b^2x^{2n}\}\,e^{-a^2x^2-\frac{b^2}{x^2}}\,dx,$$

so that

$$\left(\frac{d^2}{da^2}-4b^2\right)u_n = 4\,(n+1)\int_0^\infty x^{2n+2}e^{-a^2x^2-\frac{b^2}{x^2}}\,dx$$

$$= 4\,(n+1)\,u_{n+1} \quad\text{..........................}(4).$$

Thus from (3) and (4)

$$\left(\frac{d^2}{da^2}-4b^2\right)^n u = 4^n\,.\,n\,!\int_0^\infty x^{2n}e^{-a^2x^2-\frac{b^2}{x^2}}\,dx$$

$$= 4^n\,.\,n\,!\left(-\frac{1}{2a}\frac{d}{da}\right)^n u \quad\text{from (2)},$$

and, writing for $u$ its value $\dfrac{\sqrt{\pi}}{2a}e^{-2ab}$, this is

$$\left(\frac{d^2}{da^2}-4b^2\right)^n \frac{e^{-2ab}}{a} = (-)^n\,2^n\,.\,n\,!\left(\frac{1}{a}\frac{d}{da}\right)^n \frac{e^{-2ab}}{a},$$

which on writing $x$ in place of $a$, and $a$ in place of $-2b$ is the theorem (1).

I met with the theorem (1) in connecting certain solutions of the differential equation

$$\frac{d^2u}{dx^2} - a^2u = \frac{i(i+1)}{x^2}u,$$

which is a known transformation of Riccati's equation.

(6) Mr William Crookes, F.R.S., *On molecular physics in high vacua.*

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Mr O. Fisher, M.A., F.G.S., *Notes on a mammaliferous deposit at Barrington, near Cambridge.*

[Read February 10, 1879.]

Excavations in the neighbourhood of Barrington, for the purpose of obtaining the phosphatic nodules of the upper greensand, have lately revealed the fact that mammalian remains of the earlier pleistocene fauna are remarkably abundant in the superficial gravelly silt which there in places overlies the cretaceous deposits. The village of Barrington stands partly upon these quaternary deposits; and a small "coprolite" pit, which was opened to the east of, and nearly opposite to, the blacksmith's shop upon the Green, proved very rich in these remains. The larger part of the collection now in the Woodwardian Museum was however collected about half a mile further down the valley, upon land belonging to Trinity College, at a pit worked by Messrs Smith and Badcock. Here Mr Keeping, the curator of the Museum, spent about a fortnight in the autumn of 1878 in searching the locality: and the result has been a collection, consisting of about 360 Museum specimens. The spot may be recognised upon the ordnance map as being near where the final *n* in the word "Barrington" is printed. The tract of ground, in which the pit was opened, is about twenty feet above the alluvial flat where the river Rhee now runs. There is a small streamlet, running in a contrary direction to the Rhee, between the locality and Barrington Hill; so that the silty gravel is upon the highest part of the low tract between the hill and the river. The hill may be a hundred and fifty feet or thereabouts higher than it; and lies to the north of it.

The levels proceeding southward from the pit may be taken to be as follow:

| | yards. |
|---|---|
| From the mammalian deposit to the hedge, *level*, . . . . | 77 |
| From hedge to Northern branch of stream, *descent* 20 feet . | 100 |
| From Northern branch of stream to middle branch over alluvium, *level*, . . . . . . . . . . | 120 |
| From middle stream to Southern do, *level*, . . . . . | 134 |
| From Southern stream to a point near Foxton Station, *ascent* 20 feet, . . . . . . . . . . | 220 |
| | 651 |

Here we are again upon the level of the mammalian deposit, from which we took our departure. It appears therefore that the mammalian deposit belongs to a terrace about twenty feet higher than the present stream; and considering the flat general character of the district, it may be looked upon as a high-level gravel.

The composition of the deposit is of interest, as bearing evidence of its post-glacial age. The gravelly silt in which the bones occurred consisted of such materials as had been derived from the washing of the boulder clay, lower chalk, and greensand; which are the strata out of which the valley has obviously been excavated. There were many large stones, towards the base of it, some of which might weigh from twelve to sixteen pounds. We met with well rounded pebbles of quartz, quartzite, syenite, trap, jasper, and similar hard rocks, while the flints were but little rounded, being chiefly subangular gravel. The whole was imbedded in a grey sandy and clayey matrix with grains of glauconite and "coprolites" and rolled pieces of chalk marl. The pebbles of foreign derivation were so numerous that the material could not be called a flint gravel.

The bones which occurred throughout the silty gravel were remarkably abundant for a deposit of that nature. They were as numerous as in a cavern. There were many rolled fragments which were neglected; but the specimens which were brought away were for the most part unrolled, though mostly broken. A leg of the *Bos primigenius* was found with all its parts in association, and must have been united by ligaments when deposited. The species were determined by Mr Tawney, and were as follows:

1 *Ursus spelæus*,
2 *Meles taxus*,
3 *Hyæna spelæa*,
4 *Felis leo*,
5 *Cervus megaceros*,
6 ... *elaphus*,
7 ... small species,
8 *Bos primigenius*, (abundant)
9 *Bison priscus*,

10 *Hippopotamus major,* (common)
11 *Rhinoceros leptorhinus,* (common)
12 *Elephas antiquus,*
13 ... (?) *primigenius.*

The indications of the presence of Man were doubtful, and not to be relied on.

The shells were numerous, but the species few, and one alone strictly aquatic. They were

*Helix fasciolata* (or *caperata*),
... *nemoralis,*
*Succinea oblonga,*
*Pisidium amnicum.*

It is probable that the deposit belongs to the same age, and to the same gravel terrace as that in which nearly the same assemblage of species has been obtained at Barnwell, seven miles lower down the valley. We have*

At Barrington, but not recorded from Barnwell,
*Bison priscus,*
*Cervus elaphus,*
*Ursus spelæus,*
*Hyena spelæa.*

At Barnwell†, but not recorded from Barrington,
*Equus fossilis,*
*Cyrena fluminalis,*
*Unio litoralis.*

It is to be noticed that there are extensive gravel beds about Foxton on the south side of the valley, opposite to Barrington and upon about the same level, which are quite different in character; being almost entirely composed of subangular flints. The probability is that these were brought down from the hills to the south, where the upper chalk with flints and extensive deposits of gravel occur. Thus on either side of the main valley we find in the gravels the materials which the destruction of the strata bordering the valley would respectively supply.

The fauna of Barrington is that which is associated elsewhere‡ (if not there) with the earlier forms of flint implements of human manufacture. And it is quite certain that that fauna lived at Barrington in post-glacial times (for the state of the bones precludes the idea of their being derived from an older deposit). If

* For lists of Barnwell fossils, see Jukes-Browne's Sedgwick Prize Essay, 1878, p. 64.

† The *Rhinoceros* of Barnwell is *leptorhinus* not *tichorhinus* as usually stated.

‡ A fine "hache" was lately obtained by Mr A. F. Griffiths of Christ's College, from a gravel pit very near and upon the same level with the Barnwell mammaliferous gravel. *Camb. Ant. Soc.* May 27, 1878, Vol. IV. p. 177, Pl. A.

then, as has been lately maintained, the earlier forms of worked flints have been found beneath the chalky boulder clay, the conclusion must be, that no change or improvement took place in the art of flint knapping, during the enormous intervals in which that boulder clay was deposited, and such valleys as that of Barrington subsequently excavated.

The mammaliferous deposits of Barrington have been discovered since Mr Jukes-Browne published his Sedgwick Prize Essay on the post-tertiary geology of Cambridgeshire, 1878. Had they been known at that time, he would probably not have come to the conclusion that the valley of the Rhee is of later date than the other tributary valleys of the Cam *.

* p. 67.

I

# TABLE OF STRATA.

Scale, 1 inch to 16,000 feet
*Maximum European thicknesses taken*

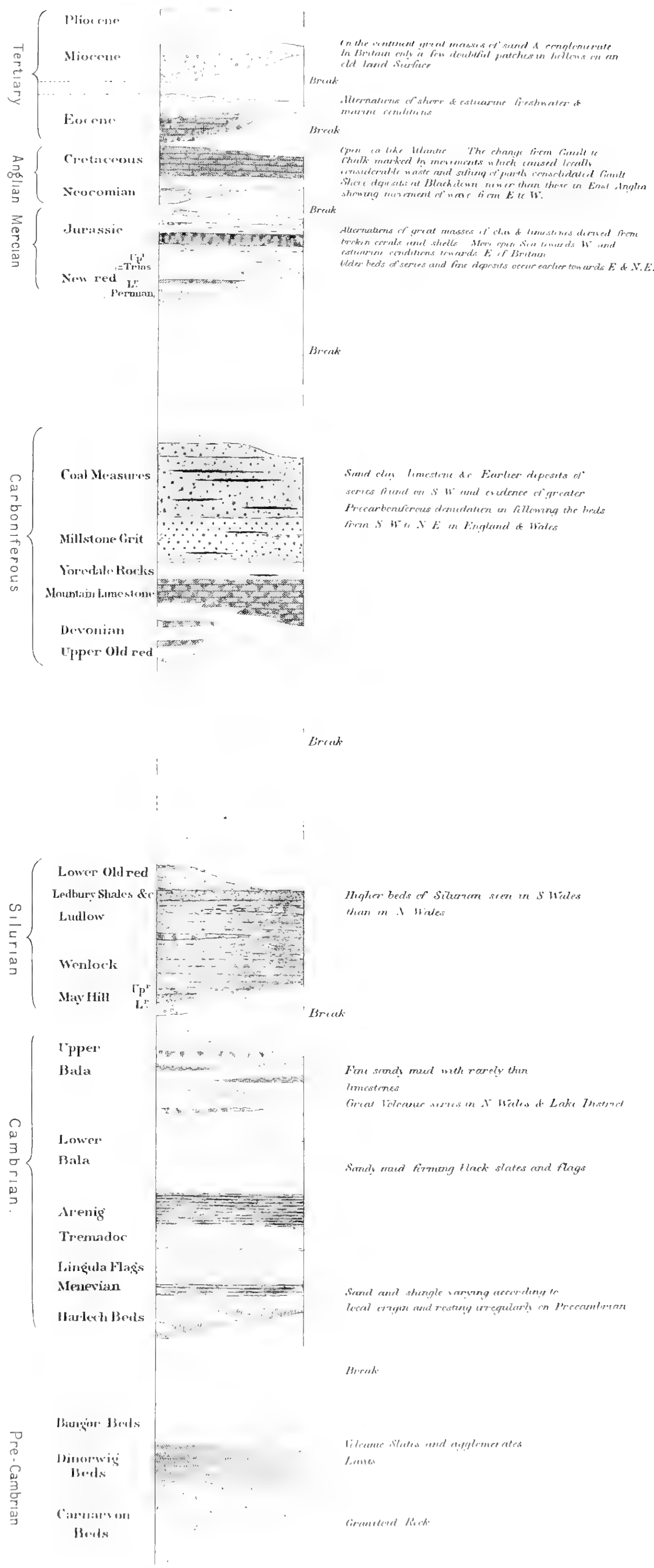


Fig. 3.

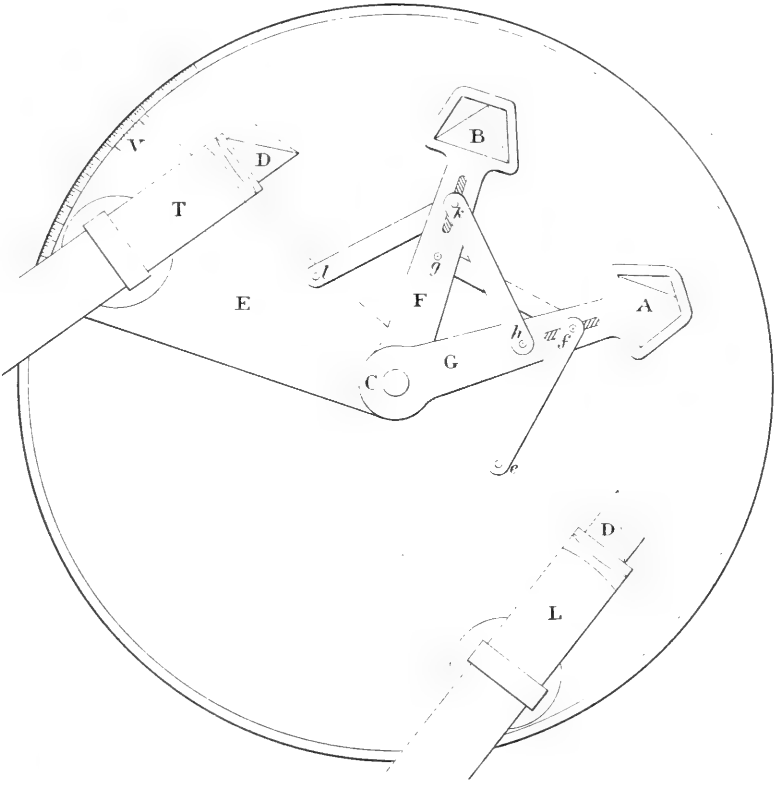



Fig. 4.

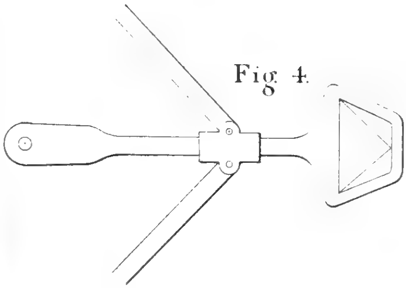


Fig. 1.

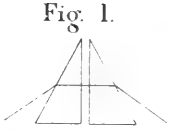
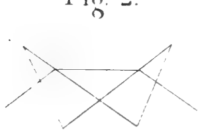


Fig. 2.



# PROCEEDINGS

OF THE

# Cambridge Philosophical Society.

## ANNUAL GENERAL MEETING.

*October* 27, 1879.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The following were elected Officers and new members of Council for the ensuing year:—

*President.*

Professor Newton.

*Vice-Presidents.*

Professor Stokes.
Professor Liveing.
Mr Michael Foster.

*Treasurer.*

Dr J. B. Pearson.

*Secretaries.*

Mr J. W. Clark.
Mr Coutts Trotter.
Mr J. W. L. Glaisher.

*New members of Council.*

Dr Phear.
Professor Hughes.
Mr W. D. Niven.

The following communications were made to the Society:—

(1) PROFESSOR CAYLEY, *Table of* $\Delta^m 0^n \div \Pi(m)$ *up to* $m=n=20$.

*(Abstract.)*

The differences of the powers of zero, $\Delta^m 0^n$, present themselves in the Calculus of Finite Differences, and especially in the applications of Herschel's theorem, $f(e^t) = f(1+\Delta)\,e^{t\cdot 0}$, for the expansion of a function of an exponential. A small table up to $\Delta^{10}0^{10}$, is given in Herschel's *Examples* (Cambridge, 1820), and is reproduced in the treatise on *Finite Differences* (1843) in the *Encyclopædia Metropolitana*. But as is known, the successive differences $\Delta 0^n$, $\Delta^2 0^n$, $\Delta^3 0^n$, ... are divisible by 1, 1.2, 1.2.3, ... and generally $\Delta^m 0^n$ is divisible by 1.2.3 ... $m$, $=\Pi(m)$; these quotients are much smaller numbers, and it is therefore desirable to tabulate them rather than the undivided differences. A table of the quotients $\Delta^m 0^n \div \Pi(m)$ up to $m=n=12$ is given by Grunert, *Crelle*, t. XXV. (1843), p. 279, but without any explanation in the heading of the meaning of the tabulated numbers $C_n^{\,k}$ $\{=\Delta^n 0^k \div \Pi(n)\}$, and without using for their determination the convenient formula $C_n^{\,+1} = nC_n^{\,k} + C^k_{n-1}$ given by Bjorling in a paper, *Crelle*, t. XXVIII. (1844), p. 284. The formula in question, say

$$\frac{\Delta^m 0^{n+1}}{\Pi(m)} = m\,\frac{\Delta^m 0^n}{\Pi(m)} + \frac{\Delta^{m-1}0^n}{\Pi(m-1)},$$

is given in the second edition (by Moulton) of Boole's *Calculus of Finite Differences* (London, 1872), p. 28, under the form

$$\Delta^m 0^n = m\,(\Delta^{m-1}0^{n-1} + \Delta^m 0^{n-1}).$$

In this paper the author extends the table of the quotients $\Delta^m 0^n \div \Pi(m)$ up to $m=n=20$, the calculation having been effected by means of the foregoing theorem.

The paper will be published *in extenso* in the *Transactions* of the Society.

(2) MR W. M. HICKS, M.A., *On the problem of two pulsating spheres in a fluid.*

1. By pulsation is meant a periodic change of volume in the same manner as by vibration is generally understood periodic change of position. The name was given by Bjerknes, who has considered the problem of the motion of two spheres in a fluid which change their volume, in a series of papers read before the Scientific Society of Christiania in 1863, 1871, and 1875*. He approximates to the apparent forces acting on the variable effective

* See also an abstract of the last in the "Repertorium der reinen und angewandten Mathematik," Bd. I.

masses of the spheres, neglecting fifth and higher powers of the ratio of the radii to the central distance. The object of the present communication is twofold, first to demonstrate a remarkable relation between the successive "mass-images" of a source in the centre of one sphere which enables us to determine rigorously the action between two spheres; and secondly, to show how this action may be applied to explain gravitation, and especially the gravitation of the vortex atoms of Sir William Thomson.

2. It is known* that if a source of fluid $P$ exist in an infinite incompressible fluid in which a sphere is at rest, that the motion of the fluid is compounded of that due to the source and a certain arrangement of a source and sinks within the sphere. The image consists of a source $\dfrac{\mu a}{OP}$ at the inverse point of $P$, and a line sink, whose line density is $-\dfrac{\mu}{a}$, thence to the centre $O$ of the sphere, where $\mu$ is the magnitude of the source at $P$ and $a$ the radius of the sphere. If now we have in a fluid two spheres $A$, $B$, of which $A$ is performing pulsations, then at any time, the motion of the fluid due to $A$ alone would be that of a source at the centre $A$ of magnitude $\mu = -a^2 \dfrac{da}{dt}$. But on account of the presence of $B$ we must suppose an image of this, as above defined, within $B$, and again an image of this within $A$, and so on indefinitely. It might seem that these images would soon become extremely complicated, but a remarkable relation holds between the successive images, which enables us to determine them all, and which we proceed to demonstrate.

It will be seen that in the image the amount of the whole line sink is equal and opposite to the source, as clearly ought to be the case, since no fluid must on the whole be generated or destroyed within the sphere. Suppose now in a fluid in which the sphere $A$ is at rest, that there exist a source of fluid $(\mu)$ at $P$, and a line sink thence to $Q$, $PQ$ being in a line with the centre of $A$, and $P$ being nearer the sphere than $Q$. Suppose also that the whole amount of the line sink is equal to the source—in other words, that the line density is $-\dfrac{\mu}{PQ}$. Then, if $P'$, $Q'$ be the inverse points of $P$, $Q$ with respect to the sphere, the image of the above arrangement will consist of (1) a source at $P' = \dfrac{\mu \,.\, a}{OP} = \mu'$; and (2) a constant line sink from $P'$ to $Q'$, whose line density is

* *Proceedings, Royal Society*, No. 197, 1879.

$-\dfrac{\mu'}{P'Q'}=-\dfrac{\mu \, . \, a}{OP \, . \, P'Q'}$; that is to say, an arrangement of precisely the same nature as the "object." For, first, the image of $\mu$ at $P$ is


$\dfrac{\mu a}{OP}$ at $P'$ and a line sink thence to $O=-\dfrac{\mu}{a}$. Secondly, considering any element of the line sink between $P$ and $Q$, its image consists of a line sink element, situated between $P'$ and $Q'$, and a line source thence to $O$. Hence from $Q'$ to $O$ there is a constant line source, the sum of all due to the line sink $PQ$. But the whole line sink $PQ=-\mu$. Therefore the line density between $O$ and $Q'$ is $\dfrac{\mu}{a}$; but that due to the source at $P$ is $-\dfrac{\mu}{a}$: so that on the whole the line density between $O$ and $Q'$ is zero.

Next, to find the line density between $P'$ and $Q'$, consider any element $dx$ of $PQ$ at a distance $x$ from $O$. From this there results $\left(\text{writing } \nu=\dfrac{\mu}{PQ}\right)$:

(1) $-\nu dx \, . \, \dfrac{a}{x}=-\dfrac{\nu \partial y}{y} \, a$ at a distance $y$, where $xy=a^2$;

(2) a line source thence towards $O=\dfrac{\nu \partial x}{a}$.

Let $\rho$ be the line density at a distance $y$, then

$$\partial\rho=\partial\left(-\frac{\nu a}{y}\right)+\frac{\nu\partial x}{a}=\frac{\nu\partial y a}{y^2}-\frac{\nu\partial y a}{y^2}=0;$$

therefore $\rho=$ constant;

or from $Q'$ to $P'$ there is a constant line density. The magnitude of this can be at once determined from the consideration that there must be no flow through the sphere, that is, that the whole amount of the line sink between $P'Q'$ must be equal and opposite to the source at $P'$. Hence it must be $-\dfrac{1}{P'Q'}\dfrac{a}{OP} \, . \, \mu$.

Since $OP \, . \, OP'=a^2$, this may be expressed $\dfrac{OP'}{P'Q'} \, . \, \dfrac{\mu}{a}$. Throughout we shall treat these *mass-images* as single wholes.

3. Passing on now to consider the force on $B$ towards $A$, let $\phi$ denote the velocity-potential, determined by the successive mass-images. The pressure at any point, taking the density of the fluid for unity, is given by

$$p = \text{const.} - \frac{\partial\phi}{\partial t} - \tfrac{1}{2}V^2,$$

and the force on $B$ from $A$ is

$$X = \int p \cos\theta \, dS,$$

the integral being taken over the sphere, and the pole of $\theta$ being $BA$. We know that $V^2$ has no effect on the resultant force,

$$\therefore\ X = -2\pi b^2 \int_0^\pi \frac{\partial\phi}{\partial t} \cos\theta \sin\theta \, d\theta.$$

Now $\phi$ is made up of a sum of velocity-potentials due to single sources and sinks. Let $\mu_n$ be one of these at a distance $r$ from $B$. Then the part of $X$ due to this is

$$2\pi b^2 \frac{d}{dt} \int_0^\pi \frac{\mu_n \cos\theta \sin\theta \, d\theta}{\sqrt{b^2 + r^2 - 2br\cos\theta}},$$

*the differentiation not extending to* b.

It can easily be shown that this is

$$\frac{2\pi}{3} b^3 \frac{d}{dt}\left(\frac{\mu_n}{r^2}\right), \qquad \text{or } \frac{2\pi}{3} \frac{d}{dt}(\mu_n r),$$

according as $\mu_n$ is outside or inside $B$.

So if $c$ be the distance between the centres of the spheres and $\rho_n$ be the distance of the source of a mass-image in $A$ from $A$, and $\rho_n'$ the distance of the other extremity, the part $X_n$ of $X$ due to this mass-image is

$$X_n = \frac{2\pi b^3}{3} \frac{d}{dt} \cdot \frac{\mu_n}{(c-\rho_n)^2} - \frac{2\pi b^3}{3} \cdot \frac{d}{dt} \int_{\rho_n'}^{\rho_n} \frac{\mu_n dx}{(\rho_n - \rho_n')(c-x)^2}$$

$$= \frac{2\pi b^3}{3} \frac{d}{dt} \frac{(\rho_n - \rho_n')\mu_n}{(c-\rho_n)^2 (c-\rho_n')},$$

and the part of $X$ due to mass-image $\nu_n$ within $B$, $\sigma_n$, $\sigma_n'$ denoting distances from $B$ is

$$X_n' = \frac{2\pi}{3} \frac{d}{dt} (\nu_n \sigma_n) - \frac{2\pi}{3} \frac{d}{dt} \int_{\sigma_n'}^{\sigma_n} \frac{\nu_n x dx}{\sigma_n - \sigma_n'}$$

$$= \frac{\pi}{3} \frac{d}{dt} \nu_n (\sigma_n - \sigma_n').$$

But if $\nu_n$ is the image of $\mu_n$,

$$\nu_n = \frac{b}{c-\rho_n}\mu_n, \quad \sigma_n = \frac{b^2}{c-\rho_n}, \quad \sigma_n' = \frac{b^2}{c-\rho_n'},$$

whence

$$X_n' = \frac{\pi}{3}\frac{d}{dt}\frac{b^3(\rho_n-\rho_n')\mu_n}{(c-\rho_n)^2(c-\rho_n')},$$

and

$$X = \pi b^3 \Sigma_0^\infty \left\{\frac{d}{dt}\frac{(\rho_n-\rho_n')\mu_n}{(c-\rho_n)^2(c-\rho_n')} + \frac{(\rho_n-\rho_n')\mu_n}{(c-\rho_n)^2(c-\rho_n')}\frac{1}{b}\frac{db}{dt}\right\}.$$

Next there is to be determined the action on $A$ from $B$. Using the results already obtained, it is easy to show that this force is

$$Y = \frac{2\pi a^3}{3}\Sigma\frac{d}{dt}\frac{(\sigma_n-\sigma_n')\nu_n}{(c-\sigma_n)^2(c-\sigma_n')} + \frac{\pi}{3}\Sigma\frac{d}{dt}(\rho_n-\rho_n')\mu_n.$$

If $\mu_{n+1}$ be regarded as the image of $\nu_n$, it will be found, since the part of $Y$ due to $\mu_0$ is zero, that

$$Y = \pi a^3 \Sigma_1^\infty \left\{\frac{d}{dt}\frac{(\rho_n-\rho_n')\mu_n}{a^3} + \frac{(\rho_n-\rho_n')\mu_n}{a^3}\cdot\frac{1}{a}\frac{da}{dt}\right\}.$$

By means of formulæ in the paper before referred to* but not yet published, it is easy to express $X$, $Y$ explicitly in terms of the radii, their rates of change, and the distance of the spheres.

4. If we are content with approximations so far as to include three images in $B$ and in $A$; $X$, $Y$ may easily be determined without reference to the general formulæ.

For writing $r$ now for $c$

$$\rho_0 = 0, \qquad \rho_0' = \infty,$$

$$\rho_1 = \frac{a^2 r}{r^2-b^2}, \qquad \rho_1' = \frac{a^2}{r},$$

$$\rho_2 = \frac{a^2 r(r^2-a^2-b^2)}{(r^2-b^2)^2-a^2r^2}, \qquad \rho_2' = \frac{a^2(r^2-a^2)}{r(r^2-a^2-b^2)},$$

$$\mu_1 = \frac{ab}{r^2-b^2}\mu_0, \qquad \mu_2 = \frac{a^2b^2}{(r^2-b^2)^2-a^2r^2}\mu_0,$$

and $$\mu_0 = a^2\frac{da}{dt} = \frac{1}{4\pi}\frac{dm_1}{dt},$$

where $m_1$ is the mass of the fluid displaced by $A$ at any time. Substituting these values

$$X = \frac{b^2}{4r^2}\frac{d(M_2)}{dt},$$

$$Y = \frac{a^2}{4r^3}\frac{dN_1}{dt},$$

* *Proceedings, Royal Society*, No. 197.

where

$$M_2 = b\frac{dm_1}{dt}\left[1 + \frac{a^3b^3}{(r^2-a^2)(r^2-a^2-b^2)^2} + \frac{a^6b^6}{\{(r^2-a^2)^2-b^2r^2\}\{(r^2-a^2)^2+(r^2-b^2)^2-(r^4-a^2b^2)\}^2}\right],$$

$$N_1 = b^3a\frac{dm_1}{dt}\left[\frac{r^2}{(r^2-b^2)^2} + \frac{a^3b^3r^2}{(r^2-a^2-b^2)\{(r^2-b^2)^2-a^2r^2\}^2}\right].$$

So interchanging $a$, $b$, the force on $A$ from $B$ due to the pulsations of $B$ is

$$X' = \frac{a^2}{4r^2}\frac{dM_1}{dt},$$

and on $B$ from $A$ is

$$\frac{b^2}{4r^3}\frac{dN_2}{dt};$$

where $M_1$, $N_2$ are $M_2$. $N_1$ with $a.b$ &c. interchanged. Finally then when both pulsate the force on $A$ from $B$ is

$$F_1 = \frac{a^2}{4r^2}\left\{\frac{dM_1}{dt} + \frac{1}{r}\frac{dN_1}{dt}\right\},$$

and on $B$ from $A$ is

$$F_2 = \frac{b^2}{4r^2}\left\{\frac{dM_2}{dt} + \frac{1}{r}\frac{dN_2}{dt}\right\},$$

or writing $\mu_1$, $\mu_2$ for $\frac{1}{4}\left(M_1 + \frac{N_1}{r}\right)$, $\frac{1}{4}\left(M_2 + \frac{N_2}{r}\right)$ respectively,

$$F_1 = \frac{a^2}{r^2}\frac{d\mu_1}{dt}, \qquad F_2 = \frac{b^2}{r^2}\cdot\frac{d\mu_2}{dt}.$$

These are the forces at any instant, the spheres being supposed held fixed. To determine the mean forces when the pulsations are quick, we have to find the mean values of $a^2\frac{d\mu_1}{dt}$ and $b^2\frac{d\mu_2}{dt}$.

If one is not pulsating, as for instance $B$, then since $b$ is constant, the mean value of $b^2\frac{d\mu_2}{dt}$ is zero. Also $M_1 = 0$, and the mean force on $A$ is of the order of the inverse cube of the distance.

5. If we confine our attention to that part of the forces depending on the inverse square of the distances,

$$\mu_1 = \frac{a}{4}\frac{dm_2}{dt} = \pi ab^2\frac{db}{dt},$$

$$\mu_2 = \frac{b}{4}\frac{dm_1}{dt} = \pi a^2b\frac{da}{dt}.$$

If $a \,.\, b$ be the mean radii, and the spheres pulsate in the same period $T$ with a difference of phase $\theta$, and with amplitudes $\alpha \,.\, \beta$ respectively, the mean values of $a^2\dfrac{d\mu_1}{dt}$ and $b^2\dfrac{d\mu_2}{dt}$ are both

$$= -\frac{4\pi^3}{T^2}\,a^2b^2\alpha\beta\cos\theta,$$

$\alpha$, $\beta$ being supposed so small that we may neglect quantities comparable to their cubes.

Hence $$F_1 = F_2 = -\frac{4\pi^3}{T^2}\cdot\frac{a^2b^2\alpha\beta}{r^2}\cos\theta,$$

and the spheres attract one another when their pulsations are on the whole concordant, and repel when not so.

As an example, if the spheres be equal, their radii 6 in., distance of centres 2 ft., the amplitude of pulsation $\frac{1}{10}$ in., the time of pulsation be $\frac{1}{10}$ sec., and the fluid water, the force is about the weight of ·42 oz.

6. The property possessed by two pulsating spheres in a fluid acting on one another with a force whose principal part varies inversely as the square of the distance, belongs also to all pulsating bodies. This follows at once from a remark made by Stokes in his paper "On some cases of Fluid Motion," where he states that whenever there is a change of volume of a body in a fluid, the velocity potential contains a term of the form $\dfrac{A}{r}$. It at once suggests itself to apply this principle to the explanation of gravitation. All we have to suppose is that atoms pulsate with a constant period, and that none have phases differing by more than 90°, or that if such once existed, they have been eliminated. The properties of a system consisting of a mixture of atoms pulsating with every possible difference of phase would be interesting to investigate: one curious property follows at once, that though of three atoms two might attract the third, they would not necessarily attract each other. If the theory offered is the true one for the explanation of gravitation, it would be possible to have celestial systems, the parts of which in each would obey the law of gravitation, but which would not influence each other, or would repel each other.

It has been pointed out, that though Thomson's theory of the vortex atom explains more properties of the atom than any other theory, yet it seems not to lend itself to any reasonable explanation of gravitation. In consequence Sir W. Thomson himself resuscitated Le Sage's theory of ultramundane corpuscles; but

the remark that such corpuscles by their battering would raise bodies to a white heat seems to contradict this explanation.

The foregoing theory would explain the gravitation of vortex atoms, if by any means it could be shown how vortex atoms in an incompressible fluid could change their volume, and pulsate in a constant periodic time. That this may be so I will show from the following considerations. Let us consider, for the sake of illustration, a single straight vortex in an infinite fluid, or rather, cyclic motion about an infinite straight line. The velocity of the fluid near the axis will be very great, and there will be therefore a small vacuum along it, of the form of a cylinder, whose radius is $\sqrt{\left(\frac{\mu^2\rho}{2\Pi}\right)}$ where $2\pi\mu$ is the *cyclic* constant, $\Pi$ the pressure of the fluid and $\rho$ its density. This will also be the case with a vortex of any form, and there *may* be a vacuum along its axis. Now suppose a vortex ring in a fluid, and suppose it has impressed on it pulsations of this vacuum (as might be done for instance by suddenly increasing the pressure $\Pi$), then it would proceed to keep up these pulsations with a constant period, which depends on the vortical constant and the pressure, and which is probably independent of the shape of the ring. If then two such vortex rings be in a fluid, and if their phases are concordant, they will attract one another inversely as the square of the distance.

Returning to the case of a straight vortex, it will be found that no pulsation will take place, or rather its period will be infinitely great; but this will not be the case with a finite vortex ring in three dimensions. In fact, suppose we have a liquid cylinder of infinite length, under the action of no forces except a uniform pressure $\Pi$ around its surface; and suppose also that in this cylinder there is cyclic motion but no vortex. It will be found that if the radius of the cylinder is $a$, that of the empty space along the axis is

$$b = \frac{\mu}{\sqrt{\left(\frac{2\Pi}{\rho} + \frac{\mu^2}{a^2}\right)}},$$

and that the time of a small pulsation is

$$2\pi\,\frac{a^2b^2}{\mu}\sqrt{\left(\frac{\log a - \log b}{a^4 - b^4}\right)}.$$

If in this we put $a = \infty$ the radius of the bore is $\sqrt{\frac{\mu^2\rho}{2\Pi}}$, and the time of pulsation becomes infinitely large. This is due to the fact, that in this case there is required an infinite impulse

to change the bore of the vacuum by a finite amount, since if a small change be produced in it, the energy of the fluid within a cylinder of radius $r$ depends on $\log r$ and is therefore infinite when $r$ is so. But in the case of a finite body in space of three dimensions the energy within a sphere of radius $r$ depends on $\frac{1}{r}$, and is therefore finite when $r$ is infinite.

In a fluid in which there are a very large number of vortex atoms, the pulsations of any one will be to some extent affected by the presence of the others. But this effect will be diminished indefinitely if the pressure of the fluid and the vortical constants be very large. It seems likely that their mutual action would tend to keep up the same time of pulsation, if any tended slightly to depart from it.

It is worth noticing that if there are a number of individual vortices in an incompressible fluid, then the fluid may not behave rigorously as such, because the vortices in it may be elastic, as is evident from what has gone before. In such a fluid pressure will not be transmitted instantaneously, and it might be thought that in this case the action between pulsating bodies might not be the inverse square. But though the action would be diminished at the same distances, yet here also at least, unless the fluid were easily compressible, it would vary as the inverse square. We may gather this from the fact that an incompressible fluid is the limit of a compressible one whose compressibility tends to zero. The most interesting point to be noted about such a fluid is that gravitation would take time for its full effect to travel any distance. It is to be clearly understood that it is not asserted that a vacuum *must* exist in every vortex atom, but only that the cyclic irrotational motion, connected with the vortex, may be so large as to produce one. To make the case clear, suppose a vortex ring in a fluid with cyclic motion through the ring. Let the pressure on the fluid at the boundary be gradually diminished; then there will be a certain point at which the pressure at the vortex will be insufficient to keep the fluid continuous, and a vacuum will arise. For instance, in the case of the cylinder considered above, we have supposed no rotational motion in the axis. There is therefore always a vacuum whatever the finite pressure on the boundary may be.

To get some idea of the magnitudes involved I have calculated the following. Suppose there are two pulsating spheres of the size of hydrogen atoms, and that the fluid has the density of the ether as estimated by Sir W. Thomson, viz. $9.36 \times 10^{-19}$. And suppose also that the amplitude of pulsation is $n$ times the radius.

Then in order that the force between two such spheres may be the same as that between two hydrogen atoms, we must have

$$\frac{n}{T'} = 7300.$$

If we suppose the amplitude $\frac{1}{50}$ of the radius, the number of pulsations in a second will be about

$$3 \cdot 65 \times 10^4.$$

The number of vibrations of the ether for the extreme red in the spectrum is about $4 \cdot 7 \times 10^{14}$.

It is to be noticed that if we suppose the ratio of the amplitude to the size of the sphere constant, then, in order that the attraction between two spheres must be proportional to the masses, the time of pulsation must be the same for all—the same condition we have already found that they should attract at all.

*November* 10, 1879.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The President referred to the great loss sustained by the Society and the University by the death of Professor Clerk Maxwell, which occurred on November 5.

The following communications were made to the Society:—

(1) Mr O. FISHER, M.A. *On implement-bearing loams in Suffolk.*

I have to relate to you this evening what I have seen under the guidance of Mr Skertchly in the district which he has rendered famous by his investigations of the loams containing worked flints. I visited a portion of this district in November, 1876, in company with Mr Belt, the naturalist, traveller, and geologist, who has since been lost to science by his early death. Mr Skertchly then conducted us over a portion of ground around Brandon, and showed us in particular three sections. It will be well to premise that this country consists principally of chalk with an undulating surface, nowhere attaining any considerable elevation. The chalk contains beds of flint of a character especially suited for the manufacture of all kinds of implements—a manufacture still carried on. The surface is covered with an incoherent brownish sand, probably spread over it by the action of wind. In some places are depressions in the surface of the chalk, which are occupied by later deposits, which have probably at one time extended beyond the depressions in which they are now seen, but have been elsewhere denuded off.

The first section to which Mr Skertchly introduced us (I am now speaking of our visit in 1876) was a brick pit at Elveden Gap. Here we saw a yellowish brown sandy brick earth beneath three or four feet of boulder clay. These beds appeared to have been preserved by having sunk into a large pot hole in the chalk before the general spread of them was denuded off. All around was a flat chalk surface, and these beds were quite surrounded by it. No implements had been obtained here.

The next section to which we were conducted was close to the Thetford water-works. Here we saw horizontally stratified brick-earth, similar to that at Elveden Gap, resting upon boulder clay. No implements were cited either from this locality. Assuming that the brickearths at these localities were originally parts of the same bed, the point made out was that this brickearth is intercalated between two boulder clays. Close by this pit, on rather higher ground, was the well of the water-works then in progress. Mr Skertchly informed us that the well passed from the brick-earth directly into the chalk, the boulder clay being absent.

The brickearth thrown out of the well was of a dark blue colour, very similar in appearance to the laminated glacial clay in the cliff at Cromer.

The third section to which we were introduced was in a brick-pit at a place called Botany Bay (being considered I suppose from its isolation a place of banishment). This is about a furlong to the west of the celebrated neolithic flint mines at Grime's Graves. Here we saw brickearth more sandy than at either of the other two places, and with the bedding much disturbed. In a lenticular thin patch of coarse carbonaceous gravel, Mr Skertchly and Mr James Geikie had found a worked flint of the scraper type, and two others were subsequently found in this pit. Here then we had implements in brickearth. But there was no evidence in this brickpit as to the position of the brickearth with regard to the boulder clay, and to my mind the brickearth differed in character from what we had seen at the former localities, both by being more sandy and disturbed in its bedding.

Mr Skertchly, however, conducted us to an old pit adjoining, where, in his opinion, he could perceive boulder clay overlapping a remnant of this brickearth. To my mind, however, the evidence was not satisfactory. Thus I came away with the conclusion that there was a brickearth in this neighbourhood, intercalated between boulder clays, and that implements had been found in brickearth, but there was no evidence to satisfy me that these brickearths were the same.

When the British Association met during last summer, Mr Skertchly brought his discoveries before the Anthropological department, and observing that the localities to which he principally

referred were not the same as those which I had seen, I determined to visit these also. I had again the invaluable advantage of Mr Skertchly himself being my guide. The first place where I saw the loam was at the brickpit at Mildenhall. The section was very badly exposed; but the brickearth could just be seen underlying a clay containing scratched pebbles of chalk, which, if not the boulder clay *in situ*, was evidently immediately derived from it. I was informed that many implements had been found here, and Mr Fenton of Mildenhall has from this place a fragment of a large bone.

About a mile to the East of this place, at a spot called High Lodge hill, I was shown a good sized pit of undoubted boulder clay, with large blocks of flint and boulders of other rocks. Close below it were several old excavations where the loamy brickearth had been dug. It was covered with three or four feet of sandy flint gravel, well stratified, and Mr Skertchly informed me that many flakes and some implements had been found here in the loam. It was obvious that, had the section been fresh and clear, the now tumbled boulder clay which formed one side of the excavation would have been seen actually overlying the gravel which covered the loamy brickearth. I was now convinced of the correctness of the views of Mr Skertchly, and that the sequence at this place was (1) boulder clay, (2) sandy gravel, (3) implement bearing loam. Mr Skertchly informed me that the loam here rests upon another boulder clay, and that upon chalk. His section, as given at the British Association, was:

| | |
|---|---|
| Chalky boulder clay, | 6 ft. |
| Gravel, | 4 ,, |
| Loamy clay, | 4 ,, |
| Boulder clay, | 6 ,, |
| Chalk. | |

But the boulder clay which I saw in the adjoining pit could not have been less than fifteen feet thick.

The sandy gravel at this spot resembles in all respects gravels which I have seen in Essex, forming a part of the Middle drift of Mr Searles V. Wood, junr.

The next section to which I was taken was at a place called West Stow (on the map it is about a furlong to the East of the Southern end of the plantation called Icklingham Belt). Here was an old pit where plenty of boulder clay was seen and a patch of loam in the middle of it. The relation of the two was not very obvious on account of the tumbled condition of the section, but Mr Skertchly informed me that the boulder clay wrapped round the loam, and it appeared to me as if the loam was engaged in it. Nevertheless the stratification of the patch of loam that

I saw was horizontal. It contained a carbonaceous layer with numerous fragments of bone in a state of great decay and also fragments of *cyclas* and *lymnæa*. Here Mr Skertchly with the assistance of a labourer dug out an implement from the carbonaceous layer.

The last place to which I was led was the Culford Brick pit. This is a large pit in full work with steam machinery on a small scale. The section here is most unmistakably clear. There are about fifteen feet of solid boulder clay containing large flints being at the termination of the general spread which covers a great part of Suffolk. This boulder clay overlies a massive deposit of well stratified loamy brickearth, of a tawney colour, very pure and good. Out of this loam Mr Skertchly, in company with Mr Bennett, dug an undoubted worked flint. Had I seen this pit first, and had the worked flint been then found in it, I should never have had any doubts upon the subject. As it was, my belief was led up to the point of certainty by degrees, as I hope I have had the satisfaction of having led yours. This pit is about five miles to the North West of Bury St Edmunds, at the North West corner of Culford Park, and is easily reached from Bury. If any one should wish for ocular demonstration of the correctness of the views of Mr Skertchly, he can obtain it with perfect ease by visiting this spot from Bury.

There can be no doubt that this discovery carries back the presence of man in this district by a very long period. I had my suspicions that the glacial clay beneath which his works had been found might be nothing more than that material which I call trail, and which has the appearance of a glacial deposit but is certainly newer than our earlier river gravels. But that is not the case. The boulder clay which covers the loams which Mr Skertchly has named the "Brandon beds" is certainly a member of the great chalky boulder clay, and the loam, also resting in some places upon boulder clay, is rightly called an interglacial deposit.

I would read you a sentence from an old note-book of mine referring to a discussion which took place in the Senate-House when the British Association met here in 1862.

"Mr Godwin Austen gave an interesting summary on drift, and said that the Diluvial drift phenomena extended to 40° latitude, and at its southern extremity the drift might be seen overlapping the older fluviatile deposits, and that he was of opinion that man existed previously to that submergence, and that it was of such a catastrophe that traditions were preserved among all Northern Nations." His prevision has now been proved correct (as has happened also in another very different matter). Nor yet can we stop at the interglacial. Professor J. D. Whitney is said to have explored

beds on the Pacific coast containing human remains and the works of man which are at least as old as the pliocene of Europe*.

These implement-bearing loams or "Brandon beds" seem to have had a rather wide extension, and I think it is open to question whether the brickearth of Hoxne may not belong to them. Their character suggests that they may have been deposited by a river when overflowing its course during states of flood, and the mode in which the flakes and implements occur sporadically scattered in them looks as if they had been dropped by the people when the waters were off.

Although the implements are of such an early age, yet their character is decidedly light and refined in workmanship, more so I think than those of the next succeeding age, which are found in the drifted gravels of the high grounds which rest on boulder clay or chalk.

There are hereabouts altogether three deposits containing worked flints. (1) The oldest, the loamy brickearths (interglacial); (2) the next in age, the old high level gravels consisting of rolled pebbles of sandstone and the crystalline rocks along with an abundance of hard chalk pebbles and flints. This occurs on the highest grounds as remnants of a well-defined ridge running across the course of the valleys of the Lark and Ouse in a nearly North and South direction. I saw from beneath it, where it was stated to lie upon chalk, two large molars of *Elephas primigenius* and a tooth of *Hippopotamus* and another of a large *Bos*. These were in the possession of Mr Fenton of Mildenhall. (3) The newest deposit that contains palæolithic flints is the gravel bordering the present river courses. Neolithic implements of various types, and many of them perfectly elegant in form and workmanship, are scattered over the neighbourhood; whilst the paths and walls contain the refuse of the modern flint knapper's shop.

(2) Mr A. G. Greenhill, M.A., *On Green's function for a rectangular parallelepiped.*

Green's function is the algebraical sum of the reciprocal of the distances from the influencing point in the interior of the parallelepiped, and from the optical images of the influencing point in the faces of the parallelepiped, an image and the corresponding reciprocal of the distance being taken as positive or negative according as the image has been formed by an even or odd number of reflexions at the faces.

Take the origin at a corner of the parallelepiped, and the three edges through the corner as co-ordinates of $x$, $y$, $z$; let $a$, $b$, $c$

* Prof. O. C. Marsh, on "History and Methods of Palæontological discovery," *Nature*, Vol. xx. p. 521.

be the lengths of the edges, and $x_1, y_1, z_1$ the co-ordinates of the influencing point.

Then the co-ordinates of an image will be

$$2ma+(-1)^{m'}x_1,\quad 2nb+(-1)^{n'}y_1,\quad 2pc+(-1)^{p'}z_1,$$

and the sign of the image will be $(-1)^{m'+n'+p'}$; and all integral values from $-\infty$ to $\infty$ must be given to $m, n, p$ to obtain all the images.

Therefore Green's function, $U$ suppose,

$$=\Sigma\frac{(-1)^{m'+n'+p'}}{\sqrt{[\{2ma+(-1)^{m'}x_1-x\}^2+\{2nb+(-1)^{n'}y_1-y\}^2+\{2pc+(-1)^{h'}z_1-z\}^2]}}.$$

Now $$\frac{1}{\sqrt{w}}=\frac{1}{\sqrt{\pi}}\int_0^\infty e^{-wt}\frac{dt}{\sqrt{t}},$$

and therefore

$$U=\frac{1}{\sqrt{\pi}}\int_0^\infty\frac{dt}{\sqrt{t}}\,\Sigma\,(-1)^{m'}e^{-\{2ma+(-1)^{m'}x_1-x\}^2t}$$
$$\times\Sigma\,(-1)^{n'}e^{-\{2nb+(-1)^{n'}y_1-y\}^2t}\times\Sigma\,(-1)^{p'}e^{-\{2pc+(-1)^{p'}z_1-z\}^2t}.$$

Now $$\Sigma\,(-1)^{m'}e^{-\{2ma+(-1)^{m'}x_1-x\}^2t}$$
$$=\Sigma e^{-(2ma+x_1-x)^2t}-\Sigma e^{-(2ma-x_1-x)^2t}$$
$$=e^{-(x-x_1)^2t}\,\Sigma e^{-4m^2a^2t+4ma(x-x_1)t}-e^{-(x+x_1)^2t}\,\Sigma e^{-4m^2a^2t+4ma(x+x_1)t}.$$

Now $$\theta_3(x, q)=\Sigma q^{m^2}e^{2mxi},$$

and therefore

$$U=\frac{1}{\sqrt{\pi}}\int_0^\infty\frac{dt}{\sqrt{t}}\times$$
$$[e^{-(x-x_1)^2t}\,\theta_3\{2ai(x-x_1)t,\ e^{-4a^2t}\}-e^{-(x+x_1)^2t}\,\theta_3\{2ai(x+x_1)t,\ e^{-4a^2t}\}]$$
$$[e^{-(y-y_1)^2t}\,\theta_3\{2bi(y-y_1)t,\ e^{-4b^2t}\}-e^{-(y+y_1)^2t}\,\theta_3\{2bi(y+y_1)t,\ e^{-4b^2t}\}]$$
$$[e^{-(z-z_1)^2t}\,\theta_3\{2ci(z-z_1)t,\ e^{-4c^2t}\}-e^{-(z+z_1)^2t}\,\theta_3\{2ci(z+z_1)t,\ e^{-4c^2t}\}],$$

the expression given in Kotteritzsch's *Electrostatik.*

But $$\theta_3(xi, q)=\sqrt{\left(\frac{\pi}{\log\frac{1}{q}}\right)}\,e^{\frac{x^2}{\log\frac{1}{q}}}\,\theta_3\left(\frac{\pi x}{\log\frac{1}{q}},\ q'\right),$$

where $$\log q\log q'=\pi^2;$$

and therefore

$$\theta_3\{2ai(x-x_1)t,\ e^{-4a^2t}\}$$
$$=\frac{1}{2a}\sqrt{\left(\frac{\pi}{t}\right)}\,e^{(x-x_1)^2t}\,\theta_3\left(\pi\frac{x-x_1}{2a},\ e^{-\frac{\pi^2}{4a^2t}}\right),$$

or $$e^{-(x-x_1)^2 t}\,\theta_3\{2ai(x-x_1)t,\ e^{-4a^2t}\}$$

$$=\frac{1}{2a}\sqrt{\left(\frac{\pi}{t}\right)}\,\theta_3\left(\pi\frac{x-x_1}{2a},\ e^{-\frac{\pi^2}{4a^2t}}\right).$$

Therefore $$U=\frac{\pi}{8abc}\int_0^\infty\frac{dt}{t^2}\times$$

$$\left\{\theta_3\left(\pi\frac{x-x_1}{2a},\ e^{-\frac{\pi^2}{4a^2t}}\right)-\theta_3\left(\pi\frac{x+x_1}{2a},\ e^{-\frac{\pi^2}{4a^2t}}\right)\right\}$$

$$\left\{\theta_3\left(\pi\frac{y-y_1}{2b},\ e^{-\frac{\pi^2}{4b^2t}}\right)-\theta_3\left(\pi\frac{y+y_1}{2b},\ e^{-\frac{\pi^2}{4b^2t}}\right)\right\}$$

$$\left\{\theta_3\left(\pi\frac{z-z_1}{2c},\ e^{-\frac{\pi^2}{4c^2t}}\right)-\theta_3\left(\pi\frac{z+z_1}{2c},\ e^{-\frac{\pi^2}{4c^2t}}\right)\right\},$$

or putting $\frac{1}{t}$ for $t$,

$$U=\frac{\pi}{8abc}\int_0^\infty dt\times$$

$$\left\{\theta_3\left(\pi\frac{x-x_1}{2a},\ e^{-\frac{\pi^2}{4a^2}t}\right)-\theta_3\left(\pi\frac{x+x_1}{2a},\ e^{-\frac{\pi^2}{4a^2}t}\right)\right\}$$

$$\left\{\theta_3\left(\pi\frac{y-y_1}{2b},\ e^{-\frac{\pi^2}{4b^2}t}\right)-\vartheta_3\left(\pi\frac{y+y_1}{2b},\ e^{-\frac{\pi^2}{4b^2}t}\right)\right\}$$

$$\left\{\theta_3\left(\pi\frac{z-z_1}{2c},\ e^{-\frac{\pi^2}{4c^2}t}\right)-\theta_3\left(\pi\frac{z+z_1}{2c},\ e^{-\frac{\pi^2}{4c^2}t}\right)\right\}.$$

The function $\theta_3(x, q)$ satisfies the differential equation

$$\frac{d^2\theta_3(x,q)}{dx^2}=4\frac{d\theta_3(x,q)}{d\log\frac{1}{q}},$$

and therefore

$$\frac{d^2}{dx^2}\theta_3\left(\pi\frac{x-x_1}{2a},\ e^{-\frac{\pi^2}{4a^2}t}\right)=4\frac{d}{dt}\theta_3\left(\pi\frac{x-x_1}{2a},\ e^{-\frac{\pi^2}{4a^2}t}\right);$$

therefore $$\frac{d^2U}{dx^2}+\frac{d^2U}{dy^2}+\frac{d^2U}{dz^2}$$

$$=\frac{\pi}{2abc}\left\{\theta_3\left(\pi\frac{x-x_1}{2a},0\right)-\theta_3\left(\pi\frac{x+x_1}{2a},0\right)\right\}$$

$$\times\left\{\theta_3\left(\pi\frac{y-y_1}{2b},0\right)-\theta_3\left(\pi\frac{y+y_1}{2b},0\right)\right\}$$

$$\times\left\{\theta_3\left(\pi\frac{z-z_1}{2c},0\right)-\theta_3\left(\pi\frac{z+z_1}{2c},0\right)\right\}$$

$$-\frac{\pi}{2abc}\left\{\theta_3\left(\pi\frac{x-x_1}{2a},1\right)-\theta_3\left(\pi\frac{x+x_1}{2a},1\right)\right\}$$

$$\times\left\{\theta_3\left(\pi\frac{y-y_1}{2b},1\right)-\theta_3\left(\pi\frac{y+y_1}{2b},1\right)\right\}$$

$$\times\left\{\theta_3\left(\pi\frac{z-z_1}{2c},1\right)-\theta_3\left(\pi\frac{z+z_1}{2c},1\right)\right\}$$

which is zero, except when $x$, $y$, $z$ are the co-ordinates of an image, when it becomes infinite.

For a single source of incompressible liquid of delivery $4\pi$ at the point $x_1y_1z_1$, under the condition that there is no flux across the faces of the parallelepiped, we must suppose the images to be all sources, and therefore the velocity function $\phi_1$

$$=\Sigma\frac{1}{\sqrt{[\{2ma+(-1)^{m'}x_1-x\}^2+\{2nb+(-1)^{n'}y_1-y\}^2+\{2pc+(-1)^{p'}z_1-z\}^2]}}$$

$$=\frac{1}{\sqrt{\pi}}\int_0^\infty\frac{dt}{\sqrt{t}}\,\Sigma e^{-\{2ma+(-1)^{m'}x_1-x\}^2t}$$

$$\times\Sigma e^{-\{2nb+(-1)^{n'}y_1-y\}^2t}\times\Sigma e^{-\{2pc+(-1)^{p'}z_1-z\}^2t}$$

$$=\frac{1}{\sqrt{\pi}}\int_0^\infty\frac{dt}{\sqrt{t}}$$

$$[e^{-(x-x_1)^2t}\,\theta_3\{2ai(x-x_1)t,q_1\}+e^{-(x+x_1)^2t}\,\theta_3\{2ai(x+x_1)t,q_1\}]$$

$$[e^{-(y-y_1)^2t}\,\theta_3\{2bi(y-y_1)t,q_2\}+e^{-(y+y_1)^2t}\,\theta_3\{2bi(y+y_1)t,q_2\}]$$

$$[e^{-(z-z_1)^2t}\,\theta_3\{2ci(z-z_1)t,q_3\}+e^{-(z+z_1)^2t}\,\theta_3\{2ci(z+z_1)t,q_3\}]$$

$$= \frac{\pi}{8abc}\int_0^\infty dt$$

$$\times\left\{\theta_3\left(\pi\frac{x-x_1}{2a},\ e^{-\frac{\pi^2}{4a^2}t}\right)+\theta_3\left(\pi\frac{x+x_1}{2a},\ e^{-\frac{\pi^2}{4a^2}t}\right)\right\}$$

$$\times\left\{\theta_3\left(\pi\frac{y-y_1}{2b},\ e^{-\frac{\pi^2}{4b^2}t}\right)+\theta_3\left(\pi\frac{y+y_1}{2a},\ e^{-\frac{\pi^2}{4b^2}t}\right)\right\}$$

$$\times\left\{\theta_3\left(\pi\frac{z-z_1}{2c},\ e^{-\frac{\pi^2}{4c^2}t}\right)+\theta_3\left(\pi\frac{z+z_1}{2c},\ e^{-\frac{\pi^2}{4c^2}t}\right)\right\}.$$

In order that there should be no flux across the faces, a sink of discharge $4\pi$ must be placed at some point $x_2y_2z_2$ in the interior and then the velocity-function of the motion is $\phi_1-\phi_2$, where $\phi_2$ is derived from $\phi_1$ by writing $x_2y_2z_2$ for $x_1y_1z_1$.

Thus for a source and sink at the opposite corners (0, 0, 0) and $(a, b, c)$

$$\phi=\frac{\pi}{abc}\int_0^\infty \theta_3\left(\frac{\pi x}{2a},\ e^{-\frac{\pi^2}{4a^2}t}\right)\theta_3\left(\frac{\pi y}{2b},\ e^{-\frac{\pi^2}{4b^2}t}\right)\theta_3\left(\frac{\pi z}{2c},\ e^{-\frac{\pi^2}{4c^2}t}\right)dt$$

$$-\frac{\pi}{abc}\int_0^\infty \theta\left(\frac{\pi x}{2a},\ e^{-\frac{\pi^2}{4a^2}t}\right)\theta\left(\frac{\pi y}{2b},\ e^{-\frac{\pi^2}{4b^2}t}\right)\theta\left(\frac{\pi z}{2c},\ e^{-\frac{\pi^2}{4c^2}t}\right)dt.$$

By certain distributions of sources and sinks along the edges and faces of the parallelepiped, it will be possible to construct the solution of the problem of the permanent temperature of the interior when the faces are maintained at certain temperatures; also by a proper distribution of sources and sinks over a surface in the interior it will be possible to discover the motion of the liquid when set in motion by the surface and bounded by the faces.

## *November* 24, 1879.

### PROFESSOR NEWTON, PRESIDENT IN THE CHAIR.

MR ALEXANDER SCOTT, B.A., of Trinity College, was ballotted for and duly elected a Fellow of the Society.

The following communications were made to the Society:

MR A. SEDGWICK, B.A.—*A preliminary notice on the development of the kidney in its relation to the Wolffian body in the chick.*

To render clear the account of the development of the kidney, and to show its relation to the Wolffian body, it is necessary to give a short account of the development of the latter. Anteriorly

the Wolffian tubules are developed from S-shaped strings of cells, continuous with the epithelium of the body-cavity, such as Kölliker has figured on p. 201 of his *Development of man and the higher animals.* Posteriorly, from the 20th segment backwards, this is not the case.

In this region before the appearance of tubules, there may be seen in longitudinal sections or in a series of transverse sections a continuous cord of cells, separate from the peritoneal epithelium, lying just internal to the Wolffian duct, extending as far back as the opening of the Wolffian duct into the cloaca.

This cord of cells developes continuously from before backwards from the intermediate cell mass. Almost directly after its appearance, it begins to break up in front into a series of primary Wolffian tubules; and this process is continued gradually backwards as far as the 30th segment. Here the process stops. From the 30th segment to the 34th, which is about the position of the opening of the Wolffian duct into the cloaca, the cord of cells does not at this stage break up into tubules. There is in fact a break in the continuity of development. The part of the cord behind the 30th segment gives rise at a later period to the tubules of the permanent kidney. Till about the end of the fourth day this part of the cord does not undergo any marked changes, but remains continuous with the part of the Wolffian body in the 30th segment, in which secondary and tertiary tubules early make their appearance.

At about this stage (end of 4th day) the ureter appears as a dorsal diverticulum of the Wolffian duct, close to its opening into the cloaca. With the appearance of the ureter, the cord of cells begins to move dorsalwards. In consequence of this change of position, which proceeds rapidly, its anterior end ceases to be continuous with the Wolffian body, and never again comes into connection with it. The ureter as it grows forward occupies a position just external to this cord of cells, and the two together grow forward dorsalwards, overlapping the hind end of the Wolffian body. Eventually this forward growth becomes much more considerable.

Meanwhile, the ureter dilates at intervals; the cells of the cord increase largely at these points, become continuous with the wall of the duct, and eventually give rise to the tubules of the kidney. I do not propose now to go into the exact details of the development of the kidney tubules; an account of this I reserve for a more detailed paper, with figures, which I hope shortly to publish. I will content myself with saying that my observations have led me to the conclusion, in opposition to the results which Kölliker[1] and Löwe[2] have arrived at for Mammalia, that the

[1] *Entwicklungsgeschichte des Menschen u. der höheren Thiere.*
[2] *Centralblatt für die med. Wissenschaften*, Oct. 1879.

cord or blastema of cells, the development of which I have just described, and which I have traced into relation with the developing ureter, gives rise not only to the vascular and connective tissue elements of the kidney, but also to the epithelium of the secretory tubules.

The fact which my observations have brought out most clearly, and on which I wish most strongly to insist, is this—The cells from which the kidney tubules arise are developed continuously and contemporaneously with the cells from which the tubules of the Wolffian body arise. It is hardly necessary to point out the important bearing of this fact on any hypothesis as to the phylogenetic origin of the kidney. It obviously points most decisively to the conclusion that the kidney is the posterior part of the Wolffian body, which has become separated from the latter, and thrown back in its later development.

Three views have been held concerning the origin of the kidney in the Amniota.

1. That it is the posterior part of the Wolffian body. (Balfour[1], Semper[2], Braun[3].)

2. That it is derived from secondary dorsal posterior tubules of the Wolffian body. (Fürbringer[4].)

3. That it is a structure sui generis which has no representative in the Ichthyopsida. (Kölliker.)

Balfour some time ago put forward the hypothesis that the kidney of the Amniota was the posterior part of the Wolffian body, and he compared it to the posterior part of the Wolffian body of Elasmobranchs, which reaches in the adult a much greater development than the anterior part, and opens by separate ducts into the cloaca. He supposed that in both cases the great development of the posterior part of the Wolffian body and its separation from the anterior part were due to the same cause, viz. the relation which the anterior part has entered into with the testis and the consequent loss of its excretory function, so that all the work of excretion was thrown upon the posterior part.

This view, arrived at by Balfour from theoretical considerations, my own observations on the chick completely confirm. Braun, who holds the same view as to the homology of the kidney of Amniota, has supported it by his observations on the development of the kidney in the Lizard.

But his observations equally well bear another interpretation, and have been used by Fürbringer in support of his view that it

[1] "Urogen. organs of Vertebrates," *Journal of Anatomy and Physiology*, Vol. x.

[2] "Urogenitalsystem der Plagiostomen," *Arbeiten*, Vol. II.

[3] "Ueber Entwickelung des Urogenitalsystems der einheimischen Reptilien." *Verh. d. phys.-med. Gesellschaft zu Würzburg.*

[4] *Morpholog. Jahrbuch*, Bd. 4.

is derived from secondary dorsal posterior tubules of the Wolffian body,

1. because it lies dorsal to the Wolffian body.
2. because according to Braun the kidney of Lizards is developed from cells which are derived as irregular solid ingrowths of the peritoneal epithelium after the primary Wolffian tubules have completely developed, and at a time corresponding to the development of the secondary dorsal tubules. My observations show that the kidney blastema of the chick does not arise as Braun has described in Lizards.

In the chick the blastema of the kidney arises contemporaneously with the primary tubules of the Wolffian body, and for the chick, at any rate, disproves one of the facts on which Fürbringer rests his suggestion. With regard to the dorsal position of the kidney, I may mention that this is easily explained as being due to its great development, which has caused it to overlap the less developed Wolffian body.

*December* 8, 1879.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:—

(1) Mr J. W. L. GLAISHER, M.A., *On the value of the constant in Legendre's formula for the number of primes inferior to a given number.*

§ 1. Legendre's formula for the number of primes inferior to a given number $x$ was given by him in the second* edition (1808) of his *Essai sur la Théorie des Nombres* (Part IV. § viii. pp. 394—398). The Chapter is entitled "D'une loi très-remarquable observée dans l'énumération des nombres premiers," and commences "Quoique la suite des nombres premiers soit extrêmement irrégulière, on peut cependant trouver avec une précision très-satisfaisante, combien il y a de ces nombres depuis 1 jusqu'à une limite donnée $x$. La formule qui résout cette question est

$$y = \frac{x}{\log x - 1\cdot08366},$$

$\log x$ étant un logarithme hyperbolique."

* The first edition was published in 1798. I have never seen a copy and do not know whether the subject of the distribution of primes is referred to in it.

Legendre then gives a table in which the values obtained from the formula are compared with the numbers actually counted up to 400,000. The interval in this table is 10,000 from 0 to 100,000 and 50,000 from 100,000 to 400,000; so that the table shows the number of primes obtained by the formula and by counting from 0 to 10,000, from 0 to 20,000, &c.; the enumerations were made from the factor table and list of primes in Vega's *Tabulæ,* which extend from 0 to 400,031. After the table Legendre remarks "Il est impossible qu'une formule représente plus fidèlement une série de nombres d'une aussi grande étendue et sujette nécessairement à de fréquentes anomalies"; but he does not give any definite information with regard to the manner in which he was led to the formula, or to assign the value 1·08366 to the constant. Legendre does indeed, at the end of the Chapter, indicate certain considerations from which he deduces that the average distance between two primes at the point $x$ in the series of numbers is of the form $A \log x + B$, and thence that the number of primes inferior to $x$ is approximately equal to

$$\frac{x}{A \log x + B - A},$$

"ce qui s'accorde avec la formule générale donnée ci-dessus, en prenant $A = 1$, $B = -0{\cdot}08366$"; the reasoning however by which the average interval $A \log x + B$ is obtained is vague and unsatisfactory.

It would thus appear that Legendre, having been led by the analytical considerations just mentioned or otherwise, to a formula of the form

$$\frac{x}{A \log x - B},$$

determined the values of $A$ and $B$ empirically by means of the enumerations; the value of $A$ would be at once found to be unity, and since in the table the number of primes up to 10,000 is given as 1230 both by the formula and the enumeration, and the two numbers are identical for no other value of $x$, it would seem that probably the constant was mainly determined from the value $x = 10{,}000$; see § 13.

In 1816 Legendre published a supplement to the *Théorie des Nombres,* which contains (pp. 61, 62) an addition to the Chapter on the distribution of primes. This principally consists of a continuation of the table from 400,000 to 1,000,000 at intervals of 50,000. The enumerations were made from Chernac's *Cribrum Arithmeticum,* published in 1811, which gives all prime factors of numbers up to 1,020,000.

In the third edition of the *Théorie des Nombres* (2 vols., 1830) the Chapter on the distribution of primes remains substantially

unaltered, but the two tables (viz. from 0 to 400,000 and from 400,000 to 1,000,000) are united in a single table*.

§ 2. In the letter written by Gauss to Encke on December 24, 1849 (*Werke*, t. II. pp. 444—447), reference is made to the value of the constant in Legendre's formula. An account of a portion of this letter and of the enumeration which accompanied it, is given *antè*, pp. 49—50.

Having, with the assistance of Goldschmidt, made an enumeration of the primes up to 3,000,000, Gauss compares the numbers for each half-million with the corresponding values of the logarithm-integral $\operatorname{li} x$, and then refers to Legendre's formula, which, he states, he had either overlooked or forgotten till Encke drew his attention to it. Gauss then gives the results of a comparison with the values obtained by Legendre's formula, and remarks that although these differences are less than in the case of the $\operatorname{li} x$ formula, they seem to increase more rapidly; he proceeds "Um Zählung und Formel in Uebereinstimmung zu bringen, müsste man respective anstatt $A = 1{\cdot}08366$ setzen

$$\begin{array}{c} 1{\cdot}09040 \\ 1{\cdot}07682 \\ 1{\cdot}07582 \\ 1{\cdot}07529 \\ 1{\cdot}07179 \\ 1{\cdot}07297 \end{array}$$

"Es scheint, dass bei wachsendem $n$ der (Durchschnitts-)Werth von $A$ abnimmt, ob aber die Grenze beim Wachsen des $n$ ins Unendliche 1 oder eine von 1 verschiedene Grösse sein wird, darüber wage ich keine Vermuthung."

Professor Tchebycheff subsequently proved in his memoir "Sur la fonction qui détermine la totalité des nombres premiers inférieurs à une limite donnée"†, that if $f(x)$ denotes the number of primes inferior to $x$, then, if $\dfrac{x}{f(x)} - \log x$ has a limit, it must be

* On page 49 of this volume of *Proceedings* I have given a list of errata in this table. On comparing it however with the two tables in the second edition and its supplement, I find that one of the errata is due to a misprint; in the second edition the number of primes counted up to 300,000 is given as 25,998, and this number is misprinted 25,988 in the third edition. But for this misprint the number of primes between 250,000 and 300,000 would appear as 3953 which is correct, and the number between 300,000 and 350,000 as 3979 which is in error by only a unit. The two errata of 10 and 9 in these groups, noted on page 49, are therefore due to a real error of 1, and the misprint of an 8 for a 9.

† *Mém. de l'Acad. de St Pétersbourg* (Savans Etrangers), t. VI. (1851), pp. 141—157: reprinted in *Liouville's Journal*, t. XVII. (1852), pp. 341—365.

unity; from which it follows that in the formula

$$\frac{x}{\log x - A},$$

as $x$ approaches infinity, the limiting value of $A$ must be unity.

§ 3. The enumerations employed by Gauss were very inaccurate as appears from the lists of errors on pp. 51—52 of the present volume, and it seemed desirable to obtain the corresponding values of $A$, using the results of the enumeration of the first three millions given on p. 48. The following table shows the values of $\phi(x)$, the actual number of primes inferior to $x$, and also the values used by Gauss:—

TABLE I.

| $x$. | $\phi(x)$*. | Value of $\phi(x)$ used by Gauss. | Difference. |
|---|---|---|---|
| 500,000 | 41,539 | 41,556 | + 17 |
| 1,000,000 | 78,499 | 78,501 | + 2 |
| 1,500,000 | 114,152 | 114,112 | − 40 |
| 2,000,000 | 148,932 | 148,883 | − 49 |
| 2,500,000 | 183,073 | 183,016 | − 57 |
| 3,000,000 | 216,817 | 216,745 | − 72 |

Taking the values in the $\phi(x)$ column, the corresponding values of $A$ (*i.e.* the values of $A$ which are such that the number given by the formula is equal to the number of primes counted, for the particular value of $x$) are shown in the second column of the next table, the third column of which contains Gauss's values of $A$, and the fourth the differences.

TABLE II.

| $x$. | $A$. | Gauss's value of $A$. | Difference. |
|---|---|---|---|
| 500,000 | 1·08548 | 1·09040 | + 0·00492 |
| 1,000,000 | 1·07649 | 1·07682 | + 0·00033 |
| 1,500,000 | 1·08060 | 1·07582 | − 0·00478 |
| 2,000,000 | 1·07971 | 1·07529 | − 0·00442 |
| 2,500,000 | 1·07605 | 1·07179 | − 0·00426 |
| 3,000,000 | 1·07757 | 1·07297 | − 0·00460 |

* In the enumerations in this column unity has been counted as a prime; in Gauss's enumerations, given in the next column, it has not been included. See § 12.

It will be observed that the values of $A$ here found diminish much less rapidly than Gauss's: in the second column the largest value of $A$ is 1·085, the least is 1·076, and the range is 0·009; in the third column the largest value of $A$ is 1·090, the least is 1·072, and the range is 0·018.

§ 4. It seemed desirable to examine whether, assuming Gauss's values of $\phi(x)$, his values of $A$ were correctly deduced, and I accordingly recalculated the values of $A$ to which Gauss's values of $\phi(x)$ lead and found them to be, to seven places,

1·0904065
1·0768193
1·0759953
1·0752906
1·0717934
1·0729731

It thus appears that the value for 1,500,000 should be 1·07600 instead of 1·07582; this only changes the difference from 478 to 460. It seems not improbable that Gauss's value was 1·07599, and that in writing to Encke he inadvertently copied 82 from the figures in the line above instead of 99.

§ 5. In order to exhibit more clearly the gradual diminution of $A$, I have calculated the values of $A$ corresponding to each quarter-million in the first four millions. These values are given in the following table.

TABLE III.

| $x$. | $\phi(x)$. | $A$. | $\Delta A$. |
|---|---|---|---|
| 250,000 | 22,045 | 1·08878 | − 0·00330 |
| 500,000 | 41,539 | 1·08548 | − 0·00806 |
| 750,000 | 60,239 | 1·07742 | − 0·00093 |
| 1,000,000 | 78,499 | 1·07649 | + 0·00477 |
| 1,250,000 | 96,470 | 1·08126 | − 0·00066 |
| 1,500,000 | 114,152 | 1·08060 | − 0·00264 |
| 1,750,000 | 131,607 | 1·07796 | + 0·00175 |
| 2,000,000 | 148,932 | 1·07971 | − 0·00079 |
| 2,250,000 | 166,082 | 1·07892 | − 0·00287 |
| 2,500,000 | 183,073 | 1·07605 | + 0·00072 |
| 2,750,000 | 199,995 | 1·07677 | + 0·00080 |
| 3,000,000 | 216,817 | 1·07757 | + 0·00261 |
| 3,250,000 | 233,578 | 1·08018 | − 0·00346 |
| 3,500,000 | 250,151 | 1·07672 | + 0·00070 |
| 3,750,000 | 266,717 | 1·07742 | − 0·00260 |
| 4,000,000 | 283,146 | 1·07482 | |

The results of the numeration for the first three millions are taken from p. 48 of the present volume, and those for the fourth million from p. 44 of the Introduction to my father's "Factor table for the fourth million" (1879).

The last column, headed $\Delta A$, contains the difference between each value of $A$ and the next.

The factor tables for the fifth and sixth millions are not yet published, so that the foregoing table cannot at present be extended to 9,000,000.

§ 6. It is not difficult to see that it is possible to determine the number of primes inferior to any given number without actually forming a factor table to this extent, and counting the primes in it. This can be effected by means of the known formula for the number of numbers inferior to $x$ and not divisible by any given primes $p_1, p_2 \ldots p_n$; there are also other methods which are more suitable in the case of the multiples of large primes*. These processes are very laborious, but they have been applied by Hargreave and Meissel independently to find the value of $\phi(x)$ for

$$x = 10{,}000{,}000,$$

and by the latter alone in the case of

$$x = 100{,}000{,}000.$$

Hargreave's investigations are contained in the *Philosophical Magazine* for 1854†; he there gives 664,632 as the number of primes (excluding unity) up to 10,000,000. This does not agree with the result given by Meissel in the second volume (1870) of the *Mathematische Annalen* §, which is 664,579. In a paper ‡ in the third volume (1871) of the same journal Meissel calculates the number of primes inferior to 100,000,000 at 5,761,460.

The values of $A$ determined from these results are shown below:

TABLE IV.

| $x$. | $\phi(x)$. | $A$. |
|---|---|---|
| 10,000,000 | 664,633 (Hargreave) | 1·07220 |
| | 664,580 (Meissel) | 1·07110 |
| 100,000,000 | 5,761,461 (Meissel) | 1·06397 |

* This subject is referred to in more detail on p. 34 of the Introduction to the Fourth Million; where also references to the papers in which these methods have been used are given.

† *On the law of prime numbers*, Ser. 4, t. VIII. pp. 114—122.

§ *Ueber die Bestimmung der Primzahlenmenge innerhalb gegebener Grenzen*, pp. 636—642.

‡ *Berechnung der Menge von Primzahlen, welche innerhalb der ersten hundert Millionen natürlichen Zahlen vorkommen*, pp. 523—525.

It thus appears that even when $x$ is as large as 100,000,000 the corresponding value of $A$ is only reduced to 1·06397, so that the approach towards unity is extremely slow. Hargreave calculated also the number of primes (excluding unity) inferior to 5,000,000 at 348,527; which gives to $A$ the value 1·07890. The factor table for the fifth million will shortly be published, and this will contain the number of primes up to 5,000,000 as found by actual counting; an enumeration of the fifth million already made, but not fully verified, gives a result differing by about 15 from Hargreave's value.

§ 7. It may be observed that the value of $A$ is very sensitive, *i.e.* that a small difference in the number of primes counted causes a considerable difference in the five-decimal values of $A$. This is due to the fact that $A$ is calculated from the formula

$$\log x - \frac{x}{\phi(x)} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1),$$

both terms of which, for the values of $x$ considered, contain one figure more than their difference.

The 'sensitiveness' of $A$ is shown by Tables I. and II. of § 3, where for example a difference of only 2 in the value of $\phi(x)$ for $x = 1{,}000{,}000$ produces a difference of 0·00033 in the value of $A$, and a difference of 49 in the value of $\phi(x)$ for $x = 2{,}000{,}000$ produces a difference of 0·00442 in the value of $A$. To illustrate the loss of a figure by the subtraction in (1) we may take as examples the extreme cases of $x = 250{,}000$ and $x = 100{,}000{,}000$.

When $x = 250{,}000$,

$$\log x = 12{\cdot}4292162, \quad \frac{x}{\phi(x)} = 11{\cdot}3404400,$$

giving

$$A = 12{\cdot}4292162 - 11{\cdot}3404400 = 1{\cdot}0887762;$$

and when $x = 100{,}000{,}000$,

$$\log x = 18{\cdot}4206807, \quad \frac{x}{\phi(x)} = 17{\cdot}3567086,$$

giving

$$A = 18{\cdot}4206807 - 17{\cdot}3567086 = 1{\cdot}0639721.$$

§ 8. Since the limiting value of $A$ when $x$ is infinite is unity, it seems worth while to compare the values given by the formula

$$\frac{x}{\log x - 1}$$

with the numbers of primes counted. The results of this comparison are shown in the following table; in which the last column,

headed difference-ratio, exhibits the ratio of the difference between the number of primes counted and the number given by the formula to the former number; thus for example, to four places of decimals,

$$\frac{171 \cdot 2}{22045} = 0 \cdot 0078.$$

TABLE V.

| $x$. | $\phi(x)$. | $\frac{x}{\log x - 1}$. | Difference. | Difference-ratio. |
|---|---|---|---|---|
| 250,000 | 22,045 | 21,873·8 | − 171·2 | 0·0078 |
| 500,000 | 41,539 | 41,246·1 | − 292·9 | 0·0071 |
| 750,000 | 60,239 | 59,866·7 | − 372·3 | 0·0062 |
| 1,000,000 | 78,499 | 78,030·4 | − 468·6 | 0·0060 |
| 1,250,000 | 96,470 | 95,868·8 | − 601·2 | 0·0062 |
| 1,500,000 | 114,152 | 113,456·1 | − 695·9 | 0·0061 |
| 1,750,000 | 131,607 | 130,839·9 | − 767·1 | 0·0058 |
| 2,000,000 | 148,932 | 148,053·2 | − 878·8 | 0·0059 |
| 2,250,000 | 166,082 | 165,120·2 | − 961·8 | 0·0058 |
| 2,500,000 | 183,073 | 182,059·1 | − 1,013·9 | 0·0055 |
| 2,750,000 | 199,995 | 198,884·6 | − 1,110·4 | 0·0056 |
| 3,000,000 | 216,817 | 215,608·3 | − 1,208·7 | 0·0056 |
| 3,250,000 | 233,578 | 232,239·6 | − 1,338·4 | 0·0057 |
| 3,500,000 | 250,151 | 248,786·7 | − 1,364·3 | 0·0055 |
| 3,750,000 | 266,717 | 265,256·4 | − 1,460·6 | 0·0055 |
| 4,000,000 | 283,146 | 281,654·3 | − 1,491·7 | 0·0053 |
| 10,000,000 | 664,633 | 661,459·0 | − 3,174·0 | 0·0048 |
| | 664,580 | | − 3,121·0 | 0·0047 |
| 100,000,000 | 5,761,461 | 5,740,303·8 | − 21,157·2 | 0·0037 |

It appears from the last column of this table that the differences between the values given by the formula and the numbers of primes counted diminish relatively to the latter as $x$ increases, as should be the case.

§ 9. Gauss compared the numbers of primes obtained by his enumeration with the values given by Legendre's formula; the deviations he found are as follows:—

| $x$. | Deviation. |
|---|---|
| 500,000 | − 23·3 |
| 1,000,000 | + 42·2 |
| 1,500,000 | + 68·1 |
| 2,000,000 | + 92·8 |
| 2,500,000 | + 159·1 |
| 3,000,000 | + 167·6 |

These deviations are of course derived from Gauss's values of $\phi(x)$, which have been quoted in Table I. (§ 3), and on replacing these by the values of $\phi(x)$ used in this paper, *i.e.* by applying the differences in Table I. as corrections, we obtain the deviations

| $x$. | Deviation. |
|---|---|
| 500,000 | − 6·3 |
| 1,000,000 | + 44·2 |
| 1,500,000 | + 28·1 |
| 2,000,000 | + 43·8 |
| 2,500,000 | + 102·1 |
| 3,000,000 | + 95·6 |

In order to determine more exactly the amount of the deviations, the following table was constructed, in which the arguments are the same as in Table V.

TABLE VI.

| $x$. | $\phi(x)$. | $\dfrac{x}{\log x - 1{\cdot}08366}$. | Difference. |
|---|---|---|---|
| 250,000 | 22,045 | 22,035·1 | − 9·9 |
| 500,000 | 41,539 | 41,532·7 | − 6·3 |
| 750,000 | 60,239 | 60,269·2 | + 30·2 |
| 1,000,000 | 78,499 | 78,543·2 | + 44·2 |
| 1,250,000 | 96,470 | 96,487·9 | + 17·9 |
| 1,500,000 | 114,152 | 114,178·6 | + 26·6 |
| 1,750,000 | 131,607 | 131,663·4 | + 56·4 |
| 2,000,000 | 148,932 | 148,975·8 | + 43·8 |
| 2,250,000 | 166,082 | 166,140·2 | + 58·2 |
| 2,500,000 | 183,073 | 183,175·1 | + 102·1 |
| 2,750,000 | 199,995 | 200,095·3 | + 100·3 |
| 3,000,000 | 216,817 | 216,912·5 | + 95·5 |
| 3,250,000 | 233,578 | 233,636·4 | + 58·4 |
| 3,500,000 | 250,151 | 250,275·1 | + 124·1 |
| 3,750,000 | 266,717 | 266,835·4 | + 118·4 |
| 4,000,000 | 283,146 | 283,323·3 | + 177·3 |
| 10,000,000 | $\begin{cases} 664{,}633 \\ 664{,}580 \end{cases}$ | 665,139·7 | $\begin{cases} +\ 506{\cdot}7 \\ +\ 559{\cdot}7 \end{cases}$ |
| 100,000,000 | 5,761,461 | 5,768,003·7 | + 6542·7 |

There are two slight discrepancies between Gauss's (corrected) deviations and the corresponding deviations in this table, viz. for $x = 1{,}500{,}000$ Gauss's value is 28·1, and the value in the table is 26·6, and for $x = 3{,}000{,}000$ there is a discrepancy amounting to only 0·1. It should be stated that in the calculation of the fore-

going table and of Table V. the values of the hyperbolic logarithms used were correct to seven decimal places.

§ 10. Gauss, Hargreave *, and Tchebycheff † obtained as a formula for the number of primes inferior to $x$ the logarithm-integral $\operatorname{li} x$; and for this to agree with

$$\frac{x}{\log x - A},$$

the value of $A$ as a function of $x$ must be determined by the equation

$$\frac{x}{\log x - A} = \operatorname{li} x,$$

that is

$$A = \log x - \frac{x}{\operatorname{li} x}.$$

Replacing $\operatorname{li} x$ by its expansion in a semi-convergent series, viz.

$$\operatorname{li} x = \frac{x}{\log x}\left(1 + \frac{1}{\log x} + \frac{2\,!}{(\log x)^2} + \frac{3\,!}{(\log x)^3} + \&\text{c.}\right),$$

we have

$$\frac{x}{\operatorname{li} x} = l\left(1 + \frac{1}{l} + \frac{2}{l^2} + \frac{6}{l^3} + \&\text{c.}\right)^{-1},$$

where $l$ denotes $\log x$, whence

$$\frac{x}{\operatorname{li} x} = l - 1 - \frac{1}{l} - \frac{3}{l^2} - \frac{13}{l^3} - \frac{71}{l^4} - \&\text{c.},$$

and therefore

$$A = 1 + \frac{1}{\log x} + \frac{3}{(\log x)^2} + \frac{13}{(\log x)^3} + \frac{71}{(\log x)^4} + \&\text{c.}$$

As a first approximation we have

$$A = 1 + \frac{1}{\log x},$$

and the formula becomes

$$\frac{x}{\log x - 1 - \dfrac{1}{\log x}}.$$

* *Philosophical Magazine*, t. XXXV. (1849), pp. 36—53, and t. VIII. (1854), pp. 114—122.

† In the memoir cited in the note on p. 298: the formula, more exactly, is $\operatorname{li} x - \operatorname{li} 2$; see § 12.

It is interesting to compare the numbers given by this formula (in which $A$ is ultimately equal to unity when $x$ increases indefinitely, as should be the case) with the numbers of primes counted; and the following table was accordingly constructed in order to exhibit the deviations.

TABLE VII.

| $x$. | $\phi(x)$. | $\dfrac{x}{\log x - 1 - \dfrac{1}{\log x}}$. | Difference. |
|---|---|---|---|
| 250,000 | 22,045 | 22,028·8 | − 16·2 |
| 500,000 | 41,539 | 41,507·0 | − 32·0 |
| 750,000 | 60,239 | 60,222·1 | − 16·9 |
| 1,000,000 | 78,499 | 78,473·7 | − 25·3 |
| 1,250,000 | 96,470 | 96,395·4 | − 74·6 |
| 1,500,000 | 114,152 | 114,062·7 | − 89·3 |
| 1,750,000 | 131,607 | 131,523·9 | − 83·1 |
| 2,000,000 | 148,932 | 148,812·5 | − 119·5 |
| 2,250,000 | 166,082 | 165,952·8 | − 129·2 |
| 2,500,000 | 183,073 | 182,963·6 | − 109·4 |
| 2,750,000 | 199,995 | 199,859·5 | − 135·5 |
| 3,000,000 | 216,817 | 216,652·3 | − 164·7 |
| 3,250,000 | 233,578 | 233,351·7 | − 226·3 |
| 3,500,000 | 250,151 | 249,965·9 | − 185·1 |
| 3,750,000 | 266,717 | 266,501·7 | − 215·3 |
| 4,000,000 | 283,146 | 282,965·0 | − 181·0 |
| 10,000,000 | $\begin{cases} 664,633 \\ 664,580 \end{cases}$ | 664,184·7 | $\begin{cases} -\ 448·3 \\ -\ 395·3 \end{cases}$ |
| 100,000,000 | 5,761,461 | 5,758,247·8 | −3,213·2 |

§ 11. In the next table are collected from Tables V., VI., VII., for convenience of comparison, the differences between the results of the enumeration and the numbers given by each of the three formulæ

$$\frac{x}{\log x - 1} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(a),$$

$$\frac{x}{\log x - 1{\cdot}08366} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(b),$$

$$\frac{x}{\log x - 1 - \dfrac{1}{\log x}} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(c).$$

TABLE VIII.

| $x$. | ($a$). | ($b$). | ($c$). |
|---|---|---|---|
| 250,000 | − 171·2 | − 9·9 | − 16·2 |
| 500,000 | − 292·9 | − 6·3 | − 32·0 |
| 750,000 | − 372·3 | + 30·2 | − 16·9 |
| 1,000,000 | − 468·6 | + 44·2 | − 25·3 |
| 1,250,000 | − 601·2 | + 17·9 | − 74·6 |
| 1,500,000 | − 695·9 | + 26·6 | − 89·3 |
| 1,750,000 | − 767·1 | + 56·4 | − 83·1 |
| 2,000,000 | − 878·8 | + 43·8 | − 119·5 |
| 2,250,000 | − 961·8 | + 58·2 | − 129·2 |
| 2,500,000 | − 1,013·9 | + 102·1 | − 109·4 |
| 2,750,000 | − 1,110·4 | + 100·3 | − 135·5 |
| 3,000,000 | − 1,208·7 | + 95·5 | − 164·7 |
| 3,250,000 | − 1,338·4 | + 58·4 | − 226·3 |
| 3,500,000 | − 1,364·3 | + 124·1 | − 185·1 |
| 3,750,000 | − 1,460·6 | + 118·4 | − 215·3 |
| 4,000,000 | − 1,491·7 | + 177·3 | − 181·0 |
| 10,000,000 | { − 3,174·0 | + 506·7 | − 448·3 |
| | { − 3,121·0 | + 559·7 | − 395·3 |
| 100,000,000 | − 21,157·2 | + 6,542·7 | − 3,213·2 |

The sign + indicates that the values given by the formula are in excess of the true values, and the sign − that they are in defect.

It thus appears that the numbers derived from ($c$), even for ranges not exceeding 4,000,000, do not differ from the numbers counted by much more than do those given by Legendre's formula. The extreme errors in the latter case are − 9·9 and + 177·3, the range being 187·2, and in the former case − 16·2 and − 226·3, the range being 210·1.

§ 12. The series used for li $x$ in § 10 is semi-convergent, and all its terms are of the same sign; the resulting value of $A$ can however be derived from Theorem V. of Tchebycheff's Memoir already referred to. This theorem is that if the function which denotes the number of primes inferior to $x$ can be represented algebraically, up to quantities of the order $\dfrac{x}{(\log x)^n}$ inclusive, by means of the functions $x$, $\log x$, $e^x$, it will be represented by the formula

$$\frac{x}{\log x} + \frac{1 \,.\, x}{(\log x)^2} + \frac{1 \,.\, 2 \,.\, x}{(\log x)^3} \ldots + \frac{1 \,.\, 2 \,.\, 3 \ldots (n-1)\, x}{(\log x)^n}.$$

The use of this theorem amounts to that of the semi-convergent series in § 10.

The logarithm-integral li $x$ is defined by the equation

$$\text{li}\, x = \int_0^x \frac{du}{\log u},$$

and therefore contains an infinite element corresponding to $u = 1$; this element however is supposed to be omitted and the principal value taken. Hargreave's formula for the number of primes inferior to $x$ was li $x$, but Tchebycheff gave

$$\int_2^x \frac{du}{\log u},$$

that is $\text{li}\, x - \text{li}\, 2$, in which the infinite element is not included between the limits of the integration; the value of li 2 is however only 1·04516 ..., so that the difference between the formulæ is unimportant even for comparatively small values of $x$.

As already mentioned Gauss excluded unity in his enumeration; this appears from the number of primes inferior to 1000 which he gives as 168*. Legendre however included unity, for he gives the number of primes inferior to 10,000 as 1230; and I have followed him in including it in the values of $\phi(x)$ given in this paper.

§ 13. With regard to the manner in which Legendre obtained his constant 1·08366, it does not seem to have been determined solely from the value $x = 10{,}000$; for, if this were the case, we should have

$$A = \log x - \tfrac{10{,}000}{1230}$$
$$= 1{\cdot}08026,$$

which differs from 1·08366 by 0·00340. Legendre's formula for $x = 10{,}000$ gives 1230·51. The constant does not appear to have been determined from any single value of $x$; and it seems likely that it was so chosen as to represent as nearly as possible the results of the earlier enumerations. When Legendre subsequently obtained the enumeration for the numbers from 400,000 to 1,000,000 the values given by the formula agreed so well with the numbers of primes counted as to apparently confirm the value which had been assigned to the constant; and he had therefore no inducement to examine the question further.

In this paper I have purposely omitted any comparisons with the li $x$ formula, as it seems desirable to defer these until the

* See p. 51 *antè*. In line 7 of this page "has not counted 1 and 2 as primes" should be "has not counted both 1 and 2 as primes", as is clear from the context.

completion of the printing of the fifth and sixth millions will enable them to be extended to 9,000,000. In the case of Legendre's formula however and Gauss's remarks upon the value of the constant, which were founded upon enumerations of the first three millions, the existing data were sufficient to permit of a fairly complete discussion of the question; however, when the two millions which are wanting are published, I propose to extend the tables in this paper from 4,000,000 to 9,000,000.

In conclusion I may mention that Tchebycheff states that the difference between Legendre's formula and his own, viz.

$$\frac{x}{\log x - 1\cdot 08366} - \int_2^x \frac{du}{\log u},$$

has a minimum value for $x = 1{,}247{,}646$, but continually increases for greater values of $x$, so that it is important that comparisons between these formulæ should extend beyond 4,000,000.

(2) Mr W. N. SHAW, M.A., *On experiments with mercury electrodes.*

In the *Philosophical Magazine* of 1874 (IV. series, vol. 47) is a paper by Lippmann, translated from Poggendorf's *Annalen*, in which the author announces as the result of experiments on the electrical effects of the motion of a mercury electrode in a capillary tube, that if the circuit be closed a current is produced through the dilute sulphuric acid from the electrode with increasing surface to the other, that the quantity of electricity disengaged is proportional to the increase of surface and independent of the form, and that these effects are due to the polarization of the increasing surface by hydrogen.

These results are criticized somewhat adversely by Quincke (Pogg. *Ann.* CLIII., p. 184), who, while he refuses to admit the proportionality of the electricity disengaged to the increase of surface, attributes some considerable part of the action caused by the motion of the electrode to the state of the fluid in contact with the mercury. Quincke's experiments also shewed a current from an increasing electrode through the fluid to the other.

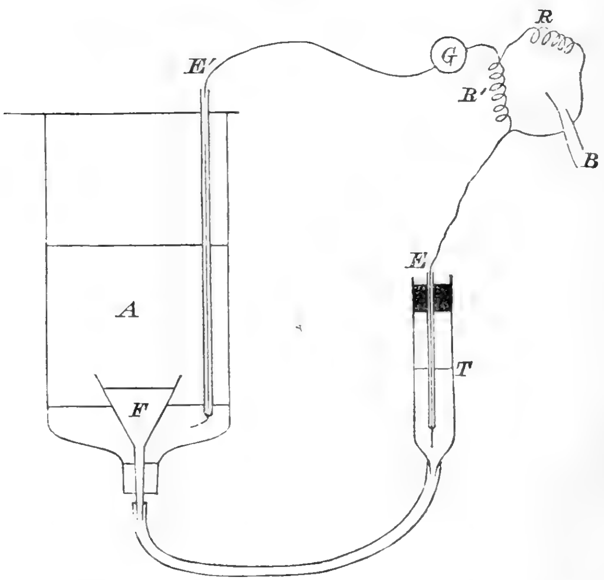
Some of Lippmann's results, as remarked by the editor of the *Philosophical Magazine*, in a foot-note, were anticipated by Varley in a paper in the *Phil. Trans.*, 1871 (p. 129). Varley there describes an apparatus in which two funnels of mercury act as electrodes in dilute sulphuric acid. One of these electrodes was polarized by hydrogen, and the two connected through a galvanometer. After the polarization current had disappeared the rocking of the apparatus caused the mercury to flow higher in one funnel and lower in the other. This gave rise, according to Varley, to a current, "the diminishing surface acting as the zinc

plate, and the increasing surface as the copper plate of a voltaic couple." This, it will be noticed, is in the opposite direction to the current observed by Lippmann and Quincke. Varley further states that if the mercury be made the positive pole of a weak battery the motion of the electrodes will no longer give rise to such currents.

I purpose laying before the Society an account of some experiments undertaken in the Physical Laboratory of the University of Berlin at the suggestion and under the direction of Prof. Helmholtz, to whom I take this opportunity of expressing my thanks, to determine whether a mercury surface can be thus completely depolarized and rendered neutral to dilute acid in contact with it, and, if so, by what electromotive force.

The apparatus employed in the first instance (see Fig. 1) consisted of an inverted bell jar $A$, in which was a funnel $F$, the stem of

Fig. 1.




which passed through the cork at the bottom of $A$, and was connected by means of an India-rubber tube with another funnel, which was in the form of a wide glass tube $T$, with the end drawn out. The tube $T$ was provided with a cork, through which passed a glass tube $E$, having a platinum wire sealed in the end within $T$. The lower part of the bell jar round $F$ contained pure freshly-distilled mercury, connected with a platinum wire sealed in a glass tube, which protected the wire from the fluid (water contain-

ing 2 p. c. by volume of Sulphuric acid), lying above the mercury and the funnel in $A$. The funnels $F$ and $E$ and the India-rubber tube contained similar mercury, the level of which could be altered by raising or lowering the funnel $T$, or if $T$ were so placed that it was filled with mercury to the cork, by raising or lowering as a piston the tube $E$, which fitted sufficiently loosely in the cork. The wires $EE'$ were connected with the electrodes of a high-resistance astatic galvanometer. An electromotive force was applied by means of the Meidinger cell $B$ acting through the resistance $R$, which could be varied at pleasure, and the magnitude of the E. M. F. was regulated by the resistance of the shunt $R'$.

Currents through the galvanometer will be denominated positive when they pass through the fluid from the mercury in the funnel $F$ to the external mercury, and negative for the opposite direction.

Whenever the apparatus just described was freshly filled, without applying any E. M. F. there was always on making contact a current through the galvanometer which varied in intensity, and was generally negative. This current disappeared when the electrodes were left in contact for several hours. When this state was arrived at, the raising of the funnel threw the galvanometer needle against its stops in the positive direction, and the reverse effect took place in lowering.

Pushing down the piston $E$ gave a mean throw of $+35^{\circ}$, and raising it a mean throw of $-32^{\circ}$. A varying electromotive force was then applied in the negative direction so as to charge the *movable electrode with Hydrogen,* time being allowed for the opposing force of polarization to attain its maximum.

The E. M. F. was gradually increased up to one quarter of the Meidinger element, and the throws of the galvanometer needle on raising and lowering the piston were in the same direction as when there was no electromotive force impressed, and increased very slightly in magnitude.

These experiments accordingly confirm the result obtained by Lippmann and Quincke as to the direction of the current, namely that a current flows from an increasing mercury electrode through the fluid to a fixed one.

The battery was then thrown out of the circuit and the electrodes connected until the polarization current was reduced to zero. The throws on raising and lowering the piston were now much smaller than originally, being approximately equal to $5^{\circ}$ each way only. An E. M. F. was then applied in the reversed direction, *i. e.* making the movable mercury surface the positive pole. The opposing E. M. F. of polarization was as before allowed to attain approximately its maximum and the needle, which was permanently deflected even for small electromotive forces, was reduced to its

mean position by means of magnets. The effects due to the raising and sinking of the piston diminished with the increasing E. M. F. until with an E. M. F. of ·2 Daniell they were no longer appreciable. But raising the whole funnel *T* produced small throws of the galvanometer until the E. M. F. was ·35 Daniell, when the only remaining effects were slight changes in the permanent current.

It would thus appear that the result described by Varley was obtained in this case for an E.M.F. of about ·35 Daniell cell.

Proceeding to confirm this result I repeated the experiments with fresh mercury and fluid, and without first depositing hydrogen on the movable mercury electrode. The result of the observations will be seen from the following table:

| E. M. F. in fractions of 1 Daniell. | Throw of needle on pushing down the piston. | Throw of needle on raising the piston. |
|---|---|---|
| 0 | $+27^\circ$ | $-24^\circ$ |
| ·01 | $+22^\circ$ | $-19^\circ$ |
| ·02 | $+19^\circ$ | $-16^\circ$ |
| ·03 | $+13^\circ{\cdot}5$ | $-12^\circ$ |
| ·04 | $+8^\circ{\cdot}5$ | $-7^\circ$ |
| ·06 | $+7^\circ$ | $-4^\circ{\cdot}5$ |
| ·08 | $+7^\circ$ | $-5^\circ$ |

At this point some of the fluid was taken out and found on testing with $H_2S$ to contain mercury. The disturbance in the fluid produced a large effect upon the current flowing through it, and it was accordingly allowed to remain over night in order to regain its previous state. On the following day observation gave for E.M.F. ·1 Daniell a throw of $+2^\circ$ in pushing down, but none on raising the piston. After this the whole funnel *T* was raised and lowered, the observations gave—

| E. M. F. | Throw on raising funnel. | Throw on lowering funnel. |
|---|---|---|
| ·12 | $+27^\circ$ | $-12^\circ$ |
| ·14 | $+25^\circ$ | $-7^\circ$ |
| ·17 | $+25^\circ$ | $-5^\circ$ |
| ·19 | $+19^\circ$ | $-4^\circ$ |
| ·21 | $+27^\circ$ | $-2^\circ$ |

On the following morning the current having passed all night

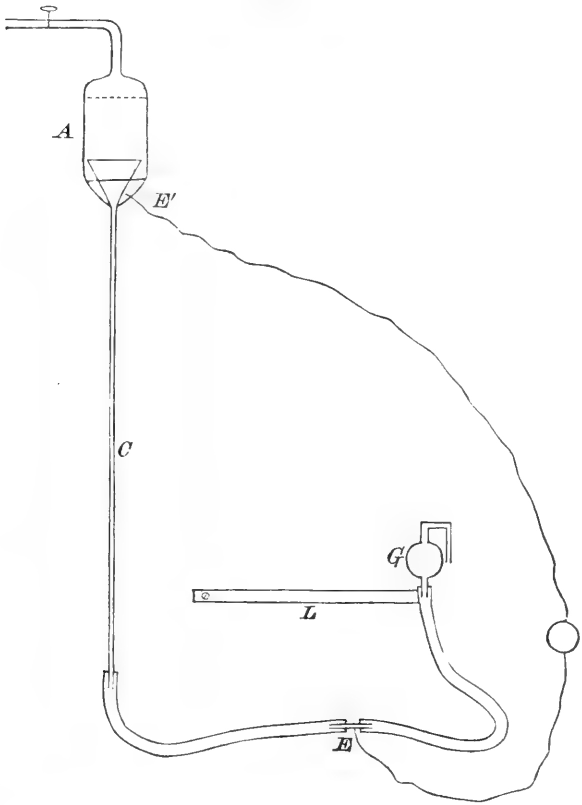
| | | |
|---|---|---|
| ·21 | $+15^\circ$ | 0 |

On Feb. 10, after the apparatus had been in use ten days, there were small white distinct crystals scattered in the movable surface of the mercury. In order to determine their nature the current was allowed to pass several days longer. The crystals were then collected, and their properties compared with those of known mercury salts, as described in Otto's *Chemistry*. They were in every particular identical with those of mercurous sulphate. The fluid also contained a large quantity of mercury in solution.

It will be noticed from this series of experiments that a much smaller E.M.F., viz. ·21 Daniell, sufficed to prevent the occurrence of the current due to the diminishing surface, but that an effect still remained when the mercury surface was increased.

In order to avoid the difficulty of the passage of so large a

Fig. 2.




permanent current, *i.e.* uncompensated by the E.M.F. of polarization, in consequence of which the needle had to be reduced to its

mean position by magnets, and which caused the solution of so much mercury, I next endeavoured to employ an electrolyte freed from air. In order to attain this an apparatus was constructed as in Fig. 2. A cylinder of glass *A*, with rounded ends, had sealed into the upper end a bent glass tube with airtight glass tap. Into the lower end a funnel with a long stem *C* was sealed. The vessel was filled with a boiling 2 p.c. solution of Sulphuric acid, made with well-boiled water, and the lower end of the stem *C*, connected by India-rubber tubing with a small glass tube *E*, fixed to the frame of the apparatus, and the tube *E* again by India-rubber tubing with the small globe *G*, fixed at the end of a lever, which was movable about a screw fixed in the frame of the apparatus. When the fluid had cooled, mercury was made to flow up the tube *C*, and partly fill the vessel *A* outside the funnel. The mercury inside and outside the funnel respectively was connected with the galvanometer by means of platinum wires sealed through the cylinder at *E′* and the glass tube at *E*. When the quantity of mercury was adjusted to its proper height in the funnel, leaving an approximately vacuous space above the electrolyte, the level could be altered, by moving the lever *L*. The vacuum thus obtained remained exceedingly good for several weeks, maintaining the fluid almost perfectly freed from air.

With this apparatus an electromotive force of ·7 Daniell was the smallest which produced any appreciable permanent current. The effects too of raising and lowering the globe were very large when only small electromotive forces were employed; for higher electromotive forces they were as under:

| E. M. F. | Throw on raising. | Throw on lowering. |
|---|---|---|
| ·6 (Daniell.) | + 28° | − 10° |
| ·75 | + 29° | − 10° |
| After remaining all night | | |
| ·75 | + 25° | − 7° |
| ·9 (1 Meidinger.) | + 28° | − 4° |

The Meidinger cell was left in circuit for the night, and a small permanent current was passing. The following morning bubbles of gas were seen *between the sides of the glass cylinder* A *and the mercury* which it contained. The electromotive force was further increased by the addition of a Daniell's element with a variable shunt and the bubbles between the mercury and glass gradually increased in size, the throw of the needle remaining approximately the same. With E.M.F. of about 1·1 Daniell there was a slow effer-

vescence of gas from the Hydrogen pole, and a throw of $18^0$ on raising and $-1^0$ on lowering.

The E.M.F. was augmented to that of five Daniells in order to obtain if possible a development of Oxygen on the movable mercury surface, the only result however was a film, presumably of sulphate, on the surface.

The apparatus was left with one Meidinger cell in circuit till the following day, when the effect of raising and lowering of the globe *was no longer appreciable.* The E.M.F. was accordingly diminished gradually in order to ascertain if the effects would increase in the same way as they had diminished. The effect however was not appreciable until the E.M.F. was reduced to ·25 Daniell, it then became sensible and increased slowly until at an E.M.F. ·05 it suddenly increased, throwing the needle against the stops on either side according as the motion of the globe was up or down. The experiments were repeated with the same apparatus, refilled with fluid, giving strictly similar results.

It appears then that there is no definite E.M.F., which by acting between the surfaces of mercury will cause the movable surface to be 'neutral' to the fluid. If the surfaces be left with a current passing from the fixed to the movable one, the throw of the galvanometer on the motion of the electrode becomes smaller the longer the current flows; and generally one may conclude that the diminution of the throw of the galvanometer is, in part at any rate, dependent on the solution of mercury of the movable electrode (which appears always to be dissolved when a current passes), and not entirely upon the E.M.F. of polarization.

(3) Mr Harry Hart, M.A., *On two models of parallel motions.*

Since the discovery by Peaucellier in 1864 of a method of producing an exact parallel motion by means of a combination of jointed rods, many solutions of the problem have been given, of which the two following are theoretically, if not practically, the most simple, inasmuch as they require the use of five links only, whereas in all other cases seven at least are employed.

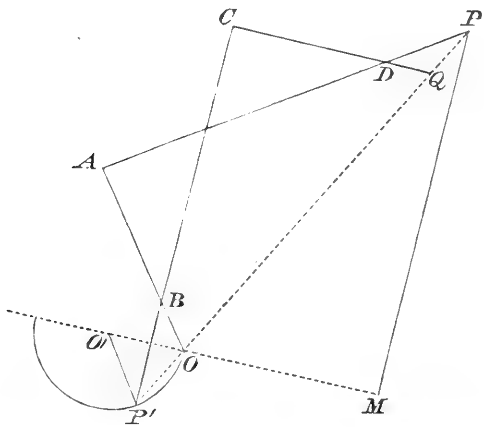
The first of these (which I described in the *Messenger of Mathematics*, vol. IV. page 82) is based on the fact that if a circle be inverted with respect to a point $O$ the inverse curve is in general a circle also, but becomes a straight line when the first circle passes through $O$.

This circle is described by means of a link capable of free motion about a fixed point $O'$, any point $P'$ on it moving in consequence in a circle whose centre is $O'$. It remains therefore to describe the Reciprocator or system of links which enables us to produce motion in a curve inverse to any given one.

Let $ABCD$ be a quadrilateral (Fig. 1) such that $AD = BC$ and $AB = CD$. Let four points $P, Q, O, P'$ be taken on the four sides $AD, CD, BA, BC$ respectively, dividing them in the same ratio, *i.e.*

$$AP : AD = CQ : CD = \&c.$$

Fig. 1.




Then it is evident that $P', O, Q, P$ are collinear, and that the three lines $AC$, $BD$, $P'OQP$ are parallel. It is further easily proved that

$$AC \cdot BD = AD^2 - AB^2,$$

or since

$$AC = \frac{AB}{BO} \cdot OP' \text{ and } BD = \frac{AB}{AO} \cdot OP$$

$$OP \cdot OP' = \frac{AO \cdot BO}{AB^2} (AD^2 - AB^2)$$

$$= AD \cdot AP - AO \cdot BO.$$

Suppose now the above four lines to represent four links jointed at $A, B, C, D$, but otherwise free, then the four-point points $P', O, Q, P$ will always be collinear, and their distances satisfy the condition

$$P'O \cdot OP \{= P'Q \cdot QP = P'O \cdot P'Q = P'Q \cdot QP\} = AD \cdot AP - AO \cdot BO$$
$$= \text{a constant}$$
$$= \mu^2 \text{ say.}$$

Thus if one of the points as $O$ be fixed and $P'$ describe any curve, then $P$ describes its inverse. Constraining $P'$ by a fifth link $O'P'$ to describe a circle, $P'$ also describes a circle, the magnitude of which $O'P'$ being invariable depends on the distance between the fixed points $O, O'$; if this $= O'P'$, as in the figure, the circle described by $P'$ passes through $O$, and the radius of its inverse becomes

infinitely great—in other words, $P$ moves in a straight line perpendicular to the line joining the fixed points $O$, $O'$.

It may be mentioned that the fourth point $Q$ describes a unicursal circular cubic (having the above line for its asymptote) which becomes the cissoid when $\mu^2 = 4r^2$, $r$ being the radius of the circle described by $P'$ (see *Messenger*, vol. IV. page 117).

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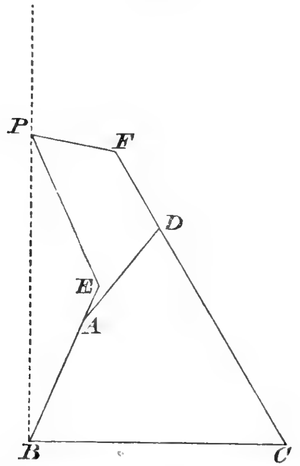
The second parallel motion was first given in the postscript of a paper which I read before the London Mathematical Society (see *Proceedings*, vol. VIII. page 288); it differs entirely from that given above, and depends on the simple property of a straight line, viz. that it is the locus of a point which moves, so that the difference between the squares of its distances from two fixed points is constant.

Let a pentagon $PEBCF$ (fig. 2) be constructed of five links, (connected only at the angular points,) whose lengths satisfy the single condition

$$PE \, . \, EB = PF \, . \, FC,$$

Fig. 2.




$BC$ being arbitrary. Let the links be placed so that the angles at $E$ and $F$ are equal, as in the figure. Take points $A$, $D$ in $EB$, $FC$, such that

$$PE : EA :: CF : FP,$$

and $$PF : FD :: BE : EP;$$

then it is easily seen that, since the triangles $PEA$, $CFP$ are similar, as are also the triangles $PFD$, $BEP$, $AD$ and $BC$ subtend equal angles at $P$, and hence that $PAD$ and $PBC$ are two similar triangles, and

$$AD : BC :: PA : PC$$
$$:: PE : FC,$$

that is $$AD = \text{const.},$$

so that if the bars $EB$, $FC$ be connected by a sixth bar

$$AD = \frac{BC \cdot PE}{PC},$$

the angles at $E$ and $F$ will be equal, and consequently since

$$PE \cdot EB = PF \cdot FC$$
$$PB^2 - PC^2 = \text{constant},$$

or $P$ describes a straight line perpendicular to $BC$. It may also be shewn that $PD^2 - PA^2$ is constant, so that $P$ describes also a straight line relatively to $AD$.

The following particular case is of special interest. Let the links be so chosen as to satisfy the conditions

$$(PE + EB)^2 + BC^2 = (PF + FC)^2,$$
$$PE = EB, \text{ and consequently } PF = FD^*.$$

By the former of these conditions we see that when $PEB$ (and therefore $PFC$) is a straight line, $PBC$ and $PDA$ are right angles, and therefore since $P$ moves perpendicularly to $BC$ and $AD$, they are always so.

The second of these conditions further shews that $PE$, $PF$ have tram motions relatively to $BC$ and $AD$, that is, if $PE$, $PF$ be produced to $Q$ and $R$, so that $E$, $F$ are the middle points of $PQ$, $PR$ respectively, $PQ$ moves, so that $P$ always lies on one fixed straight line $PB$, and $Q$ on that perpendicular to it, viz. $BC$; similarly with regard to $PR$.

Thus considering the six-bar linkage

(1) $P$ describes a straight line and any point in $PE$ or $PF$ an ellipse according as we fix $BC$ or $AD$.

(2) It is further evident that if we fix $PE$, since $B$ describes a circle and $BC$ is always perpendicular to $PB$, any point on $BC$ describes the limaçon, and for a similar reason if we fix $PF$ any point on $AD$ describes the same curve.

The two models were exhibited at the meeting.

(4) Mr J. B. Kearney, M.A., *On some results in the Theory of Equations.*

This paper related to Fourier's and Sturm's theorems, and Professor Sylvester's researches.

* It will easily be seen from these conditions that $BC = CD$ and $BA = AD$.

# PROCEEDINGS

OF THE

# Cambridge Philosophical Society.

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*February* 9, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

W. J. Lewis, M.A., Trinity College, was ballotted for and duly elected a Fellow of the Society.

The following communications were made to the Society:—

(1) Mr J. W. L. GLAISHER, M.A., *Theorems in elementary trigonometry.*

§ 1. The principal theorem referred to in the title is

$$\sin a \ \sin b \ \sin c \ \sin d \ + \cos a \ \cos b \ \cos c \ \cos d$$
$$= \sin a' \sin b' \sin c' \sin d' + \cos a' \cos b' \cos c' \cos d', \ldots .(A);$$

where

$$\begin{aligned} a' &= \tfrac{1}{2}(-a+b+c+d), \\ b' &= \tfrac{1}{2}(\ \ a-b+c+d), \\ c' &= \tfrac{1}{2}(\ \ a+b-c+d), \\ d' &= \tfrac{1}{2}(\ \ a+b+c-d), \end{aligned}$$

so that

$$a',\ b',\ c',\ d' = \sigma - a,\ \sigma - b,\ \sigma - c,\ \sigma - d,$$

where $$\sigma = \tfrac{1}{2}(a+b+c+d).$$

§ 2. If we put $d = 0$, the theorem reduces to

$$\cos a \cos b \cos c = \sin s \sin (s-a) \sin (s-b) \sin (s-c)$$
$$+ \cos s \cos (s-a) \cos (s-b) \cos (s-c) \ldots (\alpha),$$

where $$s = \tfrac{1}{2}(a+b+c);$$

this is the theorem implicitly involved in the well-known formulæ

$$\sin^2 \tfrac{1}{4}E = \frac{\sin \frac{1}{2} s \sin \frac{1}{2}(s-a) \sin \frac{1}{2}(s-b) \sin \frac{1}{2}(s-c)}{\cos \frac{1}{2} a \cos \frac{1}{2} b \cos \frac{1}{2} c},$$

$$\cos^2 \tfrac{1}{4}E = \frac{\cos \frac{1}{2} s \cos \frac{1}{2}(s-a) \cos \frac{1}{2}(s-b) \cos \frac{1}{2}(s-c)}{\cos \frac{1}{2} a \cos \frac{1}{2} b \cos \frac{1}{2} c},$$

which by division give Lhuilier's expression for the spherical excess.

It may be observed that ($\alpha$) may also be written in the form

$$\cos(b+c)\cos(c+a)\cos(a+b) = \sin a \sin b \sin c \sin(a+b+c) \\ + \cos a \cos b \cos c \cos(a+b+c).$$

§ 3. By expanding in series the sines and cosines in ($A$) and equating terms of the different orders we obtain algebraical expressions of the form: symmetrical function of $a, b, c, d$ = a similar symmetrical function of $a', b', c', d'$, viz. we thus obtain algebraical expressions of different orders involving $a, b, c, d$ symmetrically, which remain unaltered when $a, b, c, d$ are replaced by $\sigma - a$, $\sigma - b$, $\sigma - c$, $\sigma - d$.

Thus equating the second powers, we have

$$a^2 + b^2 + c^2 + d^2 = a'^2 + b'^2 + c'^2 + d'^2 \text{ ............(1)},$$

which is a well-known identity; and equating the fourth powers, we have

$$a^4 + b^4 + c^4 + d^4 + 6(b^2c^2 + c^2a^2 + a^2b^2) + 24abcd \\ = a'^4 + b'^4 + c'^4 + d'^4 + 6(b'^2c'^2 + c'^2a'^2 + a'^2b'^2) + 24a'b'c'd',$$

or, as we may write it for brevity,

$$\Sigma a^4 + 6\Sigma a^2b^2 + 24abcd = \Sigma a'^4 + 6\Sigma a'^2b'^2 + 24a'b'c'd' \text{...(2)}.$$

By squaring (1), we have

$$\Sigma a^4 + 2\Sigma a^2b^2 = \Sigma a'^4 + 2\Sigma a'^2b'^2 \text{ ..................(3)},$$

whence multiplying (3) by 3 and subtracting (2) from it,

$$\Sigma a^4 - 12abcd = \Sigma a'^4 - 12a'b'c'd' \text{ ...............(4)}.$$

Also, from (2) and (3),

$$\Sigma a^2b^2 + 6abcd = \Sigma a'^2b'^2 + 6a'b'c'd'.$$

Equating the terms of the sixth order in ($A$), we have

$$\Sigma a^6 + 15\Sigma a^4b^2 + 90\Sigma a^2b^2c^2 + 120abcd\,\Sigma a^2 \\ = \text{a similar expression involving } a', b', c', d' \text{ ........(5)}.$$

Multiplying (4) by $\Sigma a^2, \ = \Sigma a'^2$,

$$\Sigma a^6 + \Sigma a^4 b^2 - 12abcd\, \Sigma a^2 = \text{a similar expression.....(6)},$$

and, cubing (1),

$$\Sigma a^6 + 3\Sigma a^4 b^2 + 6\Sigma a^2 b^2 c^2 = \text{a similar expression.....(7)}.$$

By combining (5), (6), (7) we have

$$\Sigma a^6 + 5\Sigma a^4 b^2 = \Sigma a'^6 + 5\Sigma a'^4 b'^2,$$
$$\Sigma a^6 + 15\Sigma a^2 b^2 c^2 = \Sigma a'^6 + 15\Sigma a'^2 b'^2 c'^2,$$
$$\Sigma a^6 - 15abcd\Sigma a^2 = \Sigma a'^6 - 15a'b'c'd'\Sigma a'^2,$$

and it thus appears that the expressions

(i) $\quad \Sigma a^2$,
(ii) $\quad \Sigma a^4 + 2\Sigma a^2 b^2$,
(iii) $\quad \Sigma a^4 - 12abcd$,
(iv) $\quad \Sigma a^6 + 5\Sigma a^4 b^2$,
(v) $\quad \Sigma a^6 + 15\Sigma a^2 b^2 c^2$,
(vi) $\quad \Sigma a^6 - 15abcd\Sigma a^2$,

remain unaltered when $a, b, c, d$ are replaced by $a', b', c', d'$.

By combining these results it follows that the same is true also of the expressions

(vii) $\quad \Sigma a^2 b^2 + 6abcd$,
(viii) $\quad \Sigma a^4 b^2 - 3\Sigma a^2 b^2 c^2$,
(ix) $\quad \Sigma a^4 b^2 + 3abcd\, \Sigma a^2$,
(x) $\quad \Sigma a^2 b^2 c^2 + abcd\, \Sigma a^2$.

§ 4. It is not difficult to form other trigonometrical equations besides ($A$) in which a symmetrical function of $a, b, c, d$ is equated to a similar symmetrical function of $a', b', c', d'$. For example from the equations

$$\sin a \sin b + \sin c \sin d = \sin a' \sin b' + \sin c' \sin d',$$

$$\cos a \cos b + \cos c \cos d = \cos a' \cos b' + \cos c' \cos d',$$

which are easily verified, it follows that

$$\begin{aligned} &\sin a \sin b + \sin a \sin c + \sin a \sin d \\ &+ \sin b \sin c + \sin b \sin d + \sin c \sin d \\ = \; &\sin a' \sin b' + \sin a' \sin c' + \sin a' \sin d' \\ &+ \sin b' \sin c' + \sin b' \sin d' + \sin c' \sin d', \end{aligned}$$

$$\begin{aligned}&\cos a\ \cos b+\cos a\ \cos c+\cos a\ \cos d\\&+\cos b\ \cos c+\cos b\ \cos d+\cos c\ \cos d\\=\ &\cos a'\cos b'+\cos a'\cos c'+\cos a'\cos d'\\&+\cos b'\cos c'+\cos b'\cos d'+\cos c'\cos d',\end{aligned}$$

and equating the second, fourth, and sixth powers in these equations, we see that

$$\begin{array}{ll}\text{(xi)} & \Sigma ab,\\ \text{(xii)} & \Sigma a^3 b,\\ \text{(xiii)} & 3\Sigma a^5 b+10\Sigma a^3 b^3,\\ & \Sigma a^2,\\ & \Sigma a^4+2\Sigma a^2 b^2,\\ & \Sigma a^6+5\Sigma a^4 b^2,\end{array}$$

are unaltered by the substitution of $a'$, $b'$, $c'$, $d'$ for $a$, $b$, $c$, $d$. The last three of these expressions have been already obtained in § 3.

§ 5. It is scarcely necessary to observe that any symmetrical function of $a$ and $a'$, $b$ and $b'$, &c. such as, for example,

$$a^2(\sigma-a)^2+b^2(\sigma-b)^2+c^2(\sigma-c)^2+d^2(\sigma-d)^2$$

possesses the property of being unaltered by the substitution of $a'$, $b'$, $c'$, $d'$ for $a$, $b$, $c$, $d$, and that such expressions may be readily obtained in this manner. There would, however, generally be need of some reductions, &c. in order to deduce the simplest forms of the results.

The expression written above

$$=\Sigma a^4+2\Sigma a^2b^2+2\Sigma a^2bc-2\Sigma a^3b,$$

and since it has been shown in the last section that

$$\Sigma a^4+2\Sigma a^2b^2=\Sigma a'^4+2\Sigma a'^2b'^2,$$

and $$\Sigma a^3 b=\Sigma a'^3 b',$$

it follows that

$$\text{(xiv)}\quad \Sigma a^2bc=\Sigma a'^2b'c'.$$

It is always interesting to examine the algebraical identities to which a trigonometrical identity gives rise by expanding the sines, cosines, &c. in series and equating the terms of different orders; and, in consequence of the symmetrical form of ($A$), it seemed worth while to consider in connexion with it the algebraical results which admit of being derived from it in this manner: this has been the object of §§ 3—5. I now return to the trigonometrical formulæ which form the subject of the paper.

§ 6. It is worthy of remark that the theorem ($A$), which may be briefly written

$$\Pi(\sin a) + \Pi(\cos a) = \Pi(\sin a') + \Pi(\cos a'),$$

when squared reproduces itself. For, squaring each side,

$$\Pi(\sin^2 a) + \Pi(\cos^2 a) + \tfrac{1}{8}\Pi(\sin 2a) = \text{a similar expression},$$

that is

$$\Pi(1 - \cos 2a) + \Pi(1 + \cos 2a) + 2\Pi(\sin 2a) = \text{a similar expression},$$

viz.

$$\Sigma \cos 2a \cos 2b + \Pi(\cos 2a) + \Pi(\sin 2a) = \text{a similar expression},$$

which leads to ($A$) since, by § 4,

$$\Sigma \cos 2a \cos 2b = \Sigma \cos 2a' \cos 2b'.$$

§ 7. The other theorems referred to in the title are

$$\sin a \sin b \sin c \sin d = \sin a' \sin b' \sin c' \sin d' + \sin a'' \sin b'' \sin c'' \sin d'' \quad \ldots\ldots\ldots (B),$$

$$\cos a \cos b \cos c \cos d = \cos a' \cos b' \cos c' \cos d' - \sin a'' \sin b'' \sin c'' \sin d'' \quad \ldots\ldots\ldots (C),$$

$$\sin^2 a + \sin^2 b + \sin^2 c + \sin^2 d - \sin^2 a' - \sin^2 b' - \sin^2 c' - \sin^2 d' = -4 \sin a'' \sin b'' \sin c'' \sin d'' \quad \ldots\ldots\ldots (D),$$

$$\cos a \cos b \cos c \cos d - \sin a \sin b \sin c \sin d = 1 - \tfrac{1}{2}(\sin^2 a' + \sin^2 b' + \sin^2 c' + \sin^2 d') \quad \ldots\ldots\ldots (E);$$

where $a', b', c', d'$ are as before, viz.,

$$a' = \tfrac{1}{2}(-a + b + c + d),$$
$$b' = \tfrac{1}{2}(\ \ a - b + c + d),$$
$$c' = \tfrac{1}{2}(\ \ a + b - c + d),$$
$$d' = \tfrac{1}{2}(\ \ a + b + c - d);$$

and

$$a'' = \tfrac{1}{2}(\ \ a + b + c + d),$$
$$b'' = \tfrac{1}{2}(\ \ a + b - c - d),$$
$$c'' = \tfrac{1}{2}(\ \ a - b + c - d),$$
$$d'' = \tfrac{1}{2}(\ \ a - b - c + d).$$

The five theorems may be written more compendiously as follows:

$$\Pi(\sin a) + \Pi(\cos a) = \Pi(\sin a') + \Pi(\cos a') \ldots\ldots (A),$$
$$\Pi(\sin a) = \Pi(\sin a') + \Pi(\sin a'') \ldots\ldots (B),$$
$$\Pi(\cos a) = \Pi(\cos a') - \Pi(\sin a'') \ldots\ldots (C),$$
$$\Sigma \sin^2 a - \Sigma \sin^2 a' = -4\Pi(\sin a'') \ldots\ldots (D),$$
$$\Pi(\cos a) - \Pi(\sin a) = 1 - \tfrac{1}{2}\Sigma(\sin^2 a') \ldots\ldots (E).$$

The first theorem, $(A)$, is derivable by addition from $(B)$ and $(C)$.

§ 8. These theorems may be proved directly without difficulty: for if

$$a = \alpha + \beta,$$
$$b = \alpha - \beta,$$
$$c = \gamma + \delta,$$
$$d = \gamma - \delta,$$

then

$$a' = \gamma - \beta, \qquad a'' = \alpha + \gamma,$$
$$b' = \gamma + \beta, \qquad b'' = \alpha - \gamma,$$
$$c' = \alpha - \delta, \qquad c'' = \beta + \delta,$$
$$d' = \alpha + \delta, \qquad d'' = \beta - \delta,$$

and the theorems become

$$\begin{aligned}&\sin(\alpha+\beta)\sin(\alpha-\beta)\sin(\gamma+\delta)\sin(\gamma-\delta)\\ &\qquad + \cos(\alpha+\beta)\cos(\alpha-\beta)\cos(\gamma+\delta)\cos(\gamma-\delta)\\ &= \sin(\gamma+\beta)\sin(\gamma-\beta)\sin(\alpha+\delta)\sin(\alpha-\delta)\\ &\qquad + \cos(\gamma+\beta)\cos(\gamma-\beta)\cos(\alpha+\delta)\cos(\alpha-\delta) \ldots (A'),\end{aligned}$$

$$\begin{aligned}&\sin(\alpha+\beta)\sin(\alpha-\beta)\sin(\gamma+\delta)\sin(\gamma-\delta)\\ &\qquad = \sin(\gamma+\beta)\sin(\gamma-\beta)\sin(\alpha+\delta)\sin(\alpha-\delta)\\ &\qquad + \sin(\alpha+\gamma)\sin(\alpha-\gamma)\sin(\beta+\delta)\sin(\beta-\delta) \ldots (B'),\end{aligned}$$

$$\begin{aligned}&\cos(\alpha+\beta)\cos(\alpha-\beta)\cos(\gamma+\delta)\cos(\gamma-\delta)\\ &\qquad = \cos(\gamma+\beta)\cos(\gamma-\beta)\cos(\alpha+\delta)\cos(\alpha-\delta)\\ &\qquad - \sin(\alpha+\gamma)\sin(\alpha-\gamma)\sin(\beta+\delta)\sin(\beta-\delta) \ldots\ldots (C'),\end{aligned}$$

$$\begin{aligned}&\sin^2(\alpha+\beta) + \sin^2(\alpha-\beta) + \sin^2(\gamma+\delta) + \sin^2(\gamma-\delta)\\ &\qquad - \sin^2(\gamma+\beta) - \sin^2(\gamma-\beta) - \sin^2(\alpha+\delta) - \sin^2(\alpha-\delta)\\ &= -4\sin(\alpha+\gamma)\sin(\alpha-\gamma)\sin(\beta+\delta)\sin(\beta-\delta) \ldots\ldots (D'),\end{aligned}$$

$$\cos(\alpha+\beta)\cos(\alpha-\beta)\cos(\gamma+\delta)\cos(\gamma-\delta)$$
$$-\sin(\alpha+\beta)\sin(\alpha-\beta)\sin(\gamma+\delta)\sin(\gamma-\delta)$$
$$=1-\tfrac{1}{2}\sin^2(\gamma+\beta)-\tfrac{1}{2}\sin^2(\gamma-\beta)-\tfrac{1}{2}\sin^2(\alpha+\delta)-\tfrac{1}{2}\sin^2(\alpha-\delta) \quad \ldots\ldots\ldots (E').$$

Putting, for brevity,

$$x=\sin\alpha,$$
$$y=\sin\beta,$$
$$z=\sin\gamma,$$
$$w=\sin\delta,$$

and using the formulæ,

$$\sin(p+q)\sin(p-q)=\sin^2 p-\sin^2 q,$$
$$\cos(p+q)\cos(p-q)=1-\sin^2 p-\sin^2 q,$$

the equations $(A')$, ... $(E')$ become

$$(x^2-y^2)(z^2-w^2)+(1-x^2-y^2)(1-z^2-w^2)$$
$$=(z^2-y^2)(x^2-w^2)+(1-z^2-y^2)(1-x^2-w^2),$$

$$(x^2-y^2)(z^2-w^2)=(z^2-y^2)(x^2-w^2)+(x^2-z^2)(y^2-w^2),$$

$$(1-x^2-y^2)(1-z^2-w^2)=(1-z^2-y^2)(1-x^2-w^2)-(x^2-z^2)(y^2-w^2),$$

$$2(x^2+y^2-2x^2y^2+z^2+w^2-2z^2w^2-y^2-z^2+2y^2z^2-x^2-w^2+2x^2w^2)$$
$$=-4(x^2-z^2)(y^2-w^2),$$

$$(1-x^2-y^2)(1-z^2-w^2)-(x^2-y^2)(z^2-w^2)$$
$$=1-(y^2+z^2-2y^2z^2+x^2+w^2-2x^2w^2),$$

which identities are readily verified.

§ 9. The theorems $(A)$, ... $(E)$ are, so far as I know, new; except that $(C)$ is in effect involved in two formulæ of Lexell's, given by him in his memoir *De proprietatibus circulorum in superficie sphærica descriptorum* (*Acta Acad. Petropol.* for 1782 (1786), p. 88).

These formulæ are:—if $ABDC$ be a spherical quadrilateral inscribed in a small circle and if $AB=a$, $BD=b$, $DC=c$, $CA=d$, then

$$\sin\tfrac{1}{2}(A+D)$$
$$=\sqrt{\frac{\cos\frac{1}{4}(a+b+c+d)\cos\frac{1}{4}(a-b-c+d)\cos\frac{1}{4}(a-b+c-d)\cos\frac{1}{4}(a+b-c-d)}{\cos\frac{1}{2}a\cos\frac{1}{2}b\cos\frac{1}{2}c\cos\frac{1}{2}d}},$$

$$\cos\tfrac{1}{2}(A+D)$$
$$=-\sqrt{\frac{\sin\frac{1}{4}(a+b+c-d)\sin\frac{1}{4}(a+b-c+d)\sin\frac{1}{4}(a-b+c+d)\sin\frac{1}{4}(b+c+d-a)}{\cos\frac{1}{2}a\cos\frac{1}{2}b\cos\frac{1}{2}c\cos\frac{1}{2}d}},$$

whence by squaring and adding we have

$$\Pi(\cos\tfrac{1}{2}a)=\Pi(\cos\tfrac{1}{2}a'')+\Pi(\sin\tfrac{1}{2}a'),$$

which on changing the sign of one of the letters $a$, $b$, $c$, $d$ gives the equation $(C)$*.

§ 10. I obtained the results $(A)$, ... $(E)$ by deducing them from the elliptic function theorems

$$k'^2+k^2k'^2\,\mathrm{sn}\,a\,\mathrm{sn}\,b\,\mathrm{sn}\,c\,\mathrm{sn}\,d=M(k'^2+k^2k'^2\,\mathrm{sn}\,a'\,\mathrm{sn}\,b'\,\mathrm{sn}\,c'\,\mathrm{sn}\,d'),$$
$$k'^2-k^2\,\mathrm{cn}\,a\,\mathrm{cn}\,b\,\mathrm{cn}\,c\,\mathrm{cn}\,d=M(k'^2-k^2\,\mathrm{cn}\,a'\,\mathrm{cn}\,b'\,\mathrm{cn}\,c'\,\mathrm{cn}\,d'),$$
$$k'^2+\mathrm{dn}\,a\,\mathrm{dn}\,b\,\mathrm{dn}\,c\,\mathrm{dn}\,d=M(k'^2+\mathrm{dn}\,a'\,\mathrm{dn}\,b'\,\mathrm{dn}\,c'\,\mathrm{dn}\,d'),$$
$$k^2k'^2\,\mathrm{sn}\,a\,\mathrm{sn}\,b\,\mathrm{sn}\,c\,\mathrm{sn}\,d-k^2\,\mathrm{cn}\,a\,\mathrm{cn}\,b\,\mathrm{cn}\,c\,\mathrm{cn}\,d$$
$$=M(k'^2-\mathrm{dn}\,a'\,\mathrm{dn}\,b'\,\mathrm{dn}\,c'\,\mathrm{dn}\,d'),$$
$$k^2k'^2\,\mathrm{sn}\,a\,\mathrm{sn}\,b\,\mathrm{sn}\,c\,\mathrm{sn}\,d+\mathrm{dn}\,a\,\mathrm{dn}\,b\,\mathrm{dn}\,c\,\mathrm{dn}\,d$$
$$=M(k'^2+k^2\,\mathrm{cn}\,a'\,\mathrm{cn}\,b'\,\mathrm{cn}\,c'\,\mathrm{cn}\,d'),$$
$$k^2k'^2\,\mathrm{sn}\,a\,\mathrm{sn}\,b\,\mathrm{sn}\,c\,\mathrm{sn}\,d-k'^2$$
$$=M(k^2\,\mathrm{cn}\,a'\,\mathrm{cn}\,b'\,\mathrm{cn}\,c'\,\mathrm{cn}\,d'-\mathrm{dn}\,a'\,\mathrm{dn}\,b'\,\mathrm{dn}\,c'\,\mathrm{dn}\,d'),$$

where $a'$, $b'$, $c'$, $d'$ are as in § 1 and

$$M=\frac{\{1-k^2\,\mathrm{sn}^2\frac{1}{2}(a'+b')\,\mathrm{sn}^2\frac{1}{2}(a'-b')\}\{1-k^2\,\mathrm{sn}^2\frac{1}{2}(c'+d')\,\mathrm{sn}^2\frac{1}{2}(c'-d')\}}{\{1-k^2\,\mathrm{sn}^2\frac{1}{2}(a+b)\,\mathrm{sn}^2\frac{1}{2}(a-b)\}\{1-k^2\,\mathrm{sn}^2\frac{1}{2}(c+d)\,\mathrm{sn}^2\frac{1}{2}(c-d)\}}.$$

Expanding the expressions in these formulæ up to powers of $k^2$ inclusive, we have

$$1-k^2+k^2\Pi(\sin a)=\{1+M_0k^2\}\{1-k^2+k^2\Pi(\sin a')\},$$
$$1-k^2-k^2\Pi(\cos a)=\{1+M_0k^2\}\{1-k^2-k^2\Pi(\cos a')\},$$
$$2-k^2-\tfrac{1}{2}k^2\Sigma\sin^2 a=\{1+M_0k^2\}\{2-k^2-\tfrac{1}{2}k^2\Sigma\sin^2 a'\},$$
$$k^2\Pi(\sin a)-k^2\Pi(\cos a)=\{1+M_0k^2\}\{-k^2+\tfrac{1}{2}k^2\Sigma\sin^2 a'\},$$
$$k^2\Pi(\sin a)+1-\tfrac{1}{2}k^2\Sigma\sin^2 a=\{1+M_0k^2\}\{1-k^2+k^2\Pi(\cos a')\},$$
$$k^2\Pi(\sin a)-1+k^2=\{1+M_0k^2\}\{k^2\Pi(\cos a')-1+\tfrac{1}{2}k^2\Sigma\sin^2 a'\},$$

where $M=1+M_0k^2$ up to powers of $k^2$ inclusive.

* By a misprint in Lexell's paper, in the expression for $\sin\frac{1}{2}(A+D)$ the square-root sign only applies to the numerator; and Buzengeiger, who quotes the two formulæ from Lexell in Vol. VI. (1818) of Lindenau and Bohnenberger's *Zeitschrift für Astronomie*, p. 327, accidentally omits the square root sign from both the denominators.

These equations give, by equating the terms involving $k^2$,

$$\begin{aligned}
-1+\Pi(\sin a) &= M_0-1+\Pi(\sin a'),\\
-1-\Pi(\cos a) &= M_0-1-\Pi(\cos a'),\\
-1-\tfrac{1}{2}\Sigma\sin^2 a &= 2M_0-1-\tfrac{1}{2}\Sigma\sin^2 a',\\
\Pi(\sin a)-\Pi(\cos a) &= -1+\tfrac{1}{2}\Sigma\sin^2 a',\\
\Pi(\sin a)-\tfrac{1}{2}\Sigma\sin^2 a &= M_0-1+\Pi(\cos a'),\\
\Pi(\sin a)+1 &= -M_0+\Pi(\cos a')+\tfrac{1}{2}\Sigma\sin^2 a',
\end{aligned}$$

from which we obtain the four independent formulæ

$$\begin{aligned}
\Pi(\sin a)-\Pi(\sin a') &= M_0,\\
\Pi(\cos a)-\Pi(\cos a') &= -M_0,\\
\Sigma\sin^2 a-\Sigma\sin^2 a' &= -4M_0,\\
\Pi(\cos a)-\Pi(\sin a) &= 1-\tfrac{1}{2}\Sigma\sin^2 a'.
\end{aligned}$$

The coefficient of $k^2$ in the expansion of $M$, which has been denoted by $M_0$,

$$\begin{aligned}
&= \sin^2\tfrac{1}{2}(a+b)\sin^2\tfrac{1}{2}(a-b)+\sin^2\tfrac{1}{2}(c+d)\sin^2\tfrac{1}{2}(c-d)\\
&\qquad -\sin^2\tfrac{1}{2}(a'+b')\sin^2\tfrac{1}{2}(a'-b')-\sin^2\tfrac{1}{2}(c'+d')\sin^2\tfrac{1}{2}(c'-d')\\
&= \sin^2\tfrac{1}{2}(a-b)\{\sin^2\tfrac{1}{2}(a+b)-\sin^2\tfrac{1}{2}(c+d)\}\\
&\qquad +\sin^2\tfrac{1}{2}(c-d)\{\sin^2\tfrac{1}{2}(c+d)-\sin^2\tfrac{1}{2}(a+b)\}\\
&= \{\sin^2\tfrac{1}{2}(a+b)-\sin^2\tfrac{1}{2}(c+d)\}\{\sin^2\tfrac{1}{2}(a-b)-\sin^2\tfrac{1}{2}(c-d)\}\\
&= \sin\tfrac{1}{2}(a+b+c+d)\sin\tfrac{1}{2}(a+b-c-d)\sin\tfrac{1}{2}(a-b+c-d)\sin\tfrac{1}{2}(a-b-c+d)\\
&= \sin a''\sin b''\sin c''\sin d''\\
&= \Pi(\sin a''),
\end{aligned}$$

and substituting this value of $M_0$ the formulæ last written become identical with $(B)$, $(C)$, $(D)$, $(E)$.

§ 11. The six elliptic function theorems quoted at the beginning of the last section may be directly established without difficulty, by the same method as that employed in § 8 for the trigonometrical theorems, by means of the equations

$$\operatorname{sn}(p+q)\operatorname{sn}(p-q)=\frac{\operatorname{sn}^2 p-\operatorname{sn}^2 q}{1-k^2\operatorname{sn}^2 p\operatorname{sn}^2 q},$$

$$\operatorname{cn}(p+q)\operatorname{cn}(p-q)=\frac{1-\operatorname{sn}^2 p-\operatorname{sn}^2 q+k^2\operatorname{sn}^2 p\operatorname{sn}^2 q}{1-k^2\operatorname{sn}^2 p\operatorname{sn}^2 q},$$

$$\operatorname{dn}(p+q)\operatorname{dn}(p-q)=\frac{1-k^2\operatorname{sn}^2 p-k^2\operatorname{sn}^2 q+k^2\operatorname{sn}^2 p\operatorname{sn}^2 q}{1-k^2\operatorname{sn}^2 p\operatorname{sn}^2 q}.$$

The theorems are in effect due to Professor H. J. S. Smith, being readily deducible from the first four of the group of eleven formulæ for the multiplication of four Abelian functions given in his paper "Note on the formula for the multiplication of four theta functions" (*Proceedings of the London Mathematical Society*, Vol. x., pp. 91—100). These four formulæ are

(i) $$2\mathrm{Al}_1(a)\,\mathrm{Al}_1(b)\,\mathrm{Al}_1(c)\,\mathrm{Al}_1(d)$$

$$= \mathrm{Al}_1(a')\,\mathrm{Al}_1(b')\,\mathrm{Al}_1(c')\,\mathrm{Al}_1(d') + \frac{1}{k'^2}\,\mathrm{Al}_2(a')\,\mathrm{Al}_2(b')\,\mathrm{Al}_2(c')\,\mathrm{Al}_2(d')$$

$$+\frac{1}{k^2}\mathrm{Al}_0(a')\,\mathrm{Al}_0(b')\,\mathrm{Al}_0(c')\,\mathrm{Al}_0(d') - \frac{1}{k^2k'^2}\,\mathrm{Al}_3(a')\,\mathrm{Al}_3(b')\,\mathrm{Al}_3(c')\,\mathrm{Al}_3(d').$$

(ii) $$2\mathrm{Al}_2(a)\,\mathrm{Al}_2(b)\,\mathrm{Al}_2(c)\,\mathrm{Al}_2(d)$$

$$= \mathrm{Al}_2(a')\,\mathrm{Al}_2(b')\,\mathrm{Al}_2(c')\,\mathrm{Al}_2(d') + k'^2\,\mathrm{Al}_1(a')\,\mathrm{Al}_1(b')\,\mathrm{Al}_1(c')\,\mathrm{Al}_1(d')$$

$$+\frac{1}{k^2}\mathrm{Al}_3(a')\,\mathrm{Al}_3(b')\,\mathrm{Al}_3(c')\,\mathrm{Al}_3(d') - \frac{k'^2}{k^2}\,\mathrm{Al}_0(a')\,\mathrm{Al}_0(b')\,\mathrm{Al}_0(c')\,\mathrm{Al}_0(d').$$

(iii) $$2\mathrm{Al}_0(a)\,\mathrm{Al}_0(b)\,\mathrm{Al}_0(c)\,\mathrm{Al}_0(d)$$

$$= \mathrm{Al}_0(a')\,\mathrm{Al}_0(b')\,\mathrm{Al}_0(c')\,\mathrm{Al}_0(d') + \frac{1}{k'^2}\,\mathrm{Al}_3(a')\,\mathrm{Al}_3(b')\,\mathrm{Al}_3(c')\,\mathrm{Al}_3(d')$$

$$+k^2\,\mathrm{Al}_1(a')\,\mathrm{Al}_1(b')\,\mathrm{Al}_1(c')\,\mathrm{Al}_1(d') - \frac{k^2}{k'^2}\,\mathrm{Al}_2(a')\,\mathrm{Al}_2(b')\,\mathrm{Al}_2(c')\,\mathrm{Al}_2(d').$$

(iv) $$2\,\mathrm{Al}_3(a)\,\mathrm{Al}_3(b)\,\mathrm{Al}_3(c)\,\mathrm{Al}_3(d)$$

$$= \mathrm{Al}_3(a')\,\mathrm{Al}_3(b')\,\mathrm{Al}_3(c')\,\mathrm{Al}_3(d') + k'^2\,\mathrm{Al}_0(a')\,\mathrm{Al}_0(b')\,\mathrm{Al}_0(c')\,\mathrm{Al}_0(d')$$

$$+k^2\mathrm{Al}_2(a')\,\mathrm{Al}_2(b')\,\mathrm{Al}_2(c')\,\mathrm{Al}_2(d') - k^2k'^2\,\mathrm{Al}_1(a')\,\mathrm{Al}_1(b')\,\mathrm{Al}_1(c')\,\mathrm{Al}_1(d');$$

and, in virtue of the equations,

$$\mathrm{sn}\,x = \frac{\mathrm{Al}_1(x)}{\mathrm{Al}_0(x)},\quad \mathrm{cn}\,x = \frac{\mathrm{Al}_2(x)}{\mathrm{Al}_0(x)},\quad \mathrm{dn}\,x = \frac{\mathrm{Al}_3(x)}{\mathrm{Al}_0(x)},$$

these may be written

(i)′ $$k^2k'^2\Pi(\mathrm{sn}\,a) = \tfrac{1}{2}M\{k^2k'^2\Pi(\mathrm{sn}\,a') + k^2\Pi(\mathrm{cn}\,a') + k'^2 - \Pi(\mathrm{dn}\,a')\},$$

(ii)′ $$k^2\;\;\Pi(\mathrm{cn}\,a) = \tfrac{1}{2}M\{k^2\Pi(\mathrm{cn}\,a') + k^2k'^2\,\Pi(\mathrm{sn}\,a') + \Pi(\mathrm{dn}\,a) - k'^2\},$$

(iii)′ $$k'^2 = \tfrac{1}{2}M\{k'^2 + \Pi(\mathrm{dn}\,a') + k^2k'^2\Pi(\mathrm{sn}\,a') - k^2\Pi(\mathrm{cn}\,a')\},$$

(iv)′ $$\Pi(\mathrm{dn}\,a) = \tfrac{1}{2}M\{\Pi(\mathrm{dn}\,a') + k'^2 + k^2\Pi(\mathrm{cn}\,a') - k^2k'^2\Pi(\mathrm{sn}\,a')\},$$

where

$$M = \frac{\mathrm{Al}_0(a')\,\mathrm{Al}_0(b')\,\mathrm{Al}_0(c')\,\mathrm{Al}_0(d')}{\mathrm{Al}_0(a)\,\mathrm{Al}_0(b)\,\mathrm{Al}_0(c)\,\mathrm{Al}_0(d)};$$

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and therefore, since

$$\mathrm{Al}_0(x)\,\mathrm{Al}_0(y) = \mathrm{Al}_0{}^2\tfrac{1}{2}(x+y)\,\mathrm{Al}_0{}^2\tfrac{1}{2}(x-y) - k^2\mathrm{Al}_1{}^2\tfrac{1}{2}(x+y)\mathrm{Al}_1{}^2\tfrac{1}{2}(x-y),$$

$$M = \frac{\mathrm{Al}_0{}^2\frac{1}{2}(a'+b')\,\mathrm{Al}_0{}^2\frac{1}{2}(a'-b') - k^2\mathrm{Al}_1{}^2\frac{1}{2}(a'+b')\,\mathrm{Al}_1{}^2\frac{1}{2}(a'-b')}{\mathrm{Al}_0{}^2\frac{1}{2}(a+b)\,\mathrm{Al}_0{}^2\frac{1}{2}(a-b) - k^2\mathrm{Al}_1{}^2\frac{1}{2}(a+b)\,\mathrm{Al}_1{}^2\frac{1}{2}(a-b)}$$
$$\times\frac{\mathrm{Al}_0{}^2\frac{1}{2}(c'+d')\,\mathrm{Al}_0{}^2\frac{1}{2}(c'-d') - k^2\mathrm{Al}_1{}^2\frac{1}{2}(c'+d')\,\mathrm{Al}_1{}^2\frac{1}{2}(c'-d')}{\mathrm{Al}_0{}^2\frac{1}{2}(c+d)\,\mathrm{Al}_0{}^2\frac{1}{2}(c-d) - k^2\mathrm{Al}_1{}^2\frac{1}{2}(c+d)\,\mathrm{Al}_1{}^2\frac{1}{2}(c-d)},$$

whence, since

$$\begin{aligned}&\mathrm{Al}_0\tfrac{1}{2}(a+b)\,\mathrm{Al}_0\tfrac{1}{2}(a-b)\,\mathrm{Al}_0\tfrac{1}{2}(c+d)\,\mathrm{Al}_0\tfrac{1}{2}(c-d)\\ =\;&\mathrm{Al}_0\tfrac{1}{2}(a'+b')\,\mathrm{Al}_0\tfrac{1}{2}(a'-b')\,\mathrm{Al}_0\tfrac{1}{2}(c'+d')\,\mathrm{Al}_0\tfrac{1}{2}(c'-d'),\end{aligned}$$

$$M = \frac{\{1-k^2\mathrm{sn}^2\frac{1}{2}(a'+b')\,\mathrm{sn}^2\frac{1}{2}(a'-b')\}\{1-k^2\mathrm{sn}^2\frac{1}{2}(c'+d')\,\mathrm{sn}^2\frac{1}{2}(c'-d')\}}{\{1-k^2\mathrm{sn}^2\frac{1}{2}(a+b)\,\mathrm{sn}^2\frac{1}{2}(a-b)\}\{1-k^2\mathrm{sn}^2\frac{1}{2}(c+d)\,\mathrm{sn}^2\frac{1}{2}(c-d)\}},$$

and by combining the four formulæ (i)′...(iv)′ we obtain at once the six theorems quoted at the beginning of § 10.

(2) Mr R. T. GLAZEBROOK, M.A. *On the Reflexion and Refraction of light.*

In his paper on the reflexion and refraction of light, Green assumes that there are no forces acting on the ether but those which arise from the relative displacements of its parts. Kirchhoff, in a paper read before the Royal Academy of Berlin, considers the problem of the reflexion and refraction of plane waves in crystalline media. He supposes there are impressed forces acting over the bounding surface of the two media.

These forces are such as to prevent the propagation of normal waves either reflected or refracted, while at the same time they do no work on the vibrating particles of the ether. On the same assumptions I propose here to discuss at length the case of two isotropic media, following Kirchhoff's method.

Let the forces be $X$, $Y$, $Z$, acting per unit area of the bounding surface parallel to the axes. Let $u$, $v$, $w$ be the displacements of a particle whose position first is $x$, $y$, $z$. Then the rate at which work is done per unit area is

$$X\frac{du}{dt} + Y\frac{dv}{dt} + Z\frac{dw}{dt}.$$

Let $N_1$, $N_2$, $N_3$; $T_1$, $T_2$, $T_3$ be the strains in one medium, $N_1'$, $N_2'$, $N_3'$; $T_1'$, $T_2'$, $T_3'$ in the other, $\rho$, $\rho'$ the densities. Let the plane $x=0$ be the bounding surface of the two media, and let $x$ be negative for the first positive for the second medium.

Then the expression for the work becomes

$$(N_1 - N_1')\frac{du}{dt} + (T_3 - T_3')\frac{dv}{dt} + (T_2 - T_2')\frac{dw}{dt} \;\ldots\ldots\; (1).$$

Also $$N_1 = A\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right) - 2B\left(\frac{dv}{dy} + \frac{du}{dz}\right),$$

$$T_2 = B\left(\frac{du}{dz} + \frac{dw}{dx}\right),$$

$$T_3 = B\left(\frac{du}{dy} + \frac{dv}{dx}\right).$$

Moreover, since there are no normal waves,

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

Also $u = u'$, $v = v'$, $w = w'$, when $x = 0$.

Any of the equations at the boundary surface being true for all values of $y$, $z$ and $t$, we may differentiate with reference to $y$, $z$ or $t$.

Substituting, we have for the work

$$\left\{B\left(\frac{dw}{dx} - \frac{du}{dz}\right) - B'\left(\frac{dw'}{dx} - \frac{du'}{dz}\right)\right\}\frac{dw}{dt} + \left\{B\left(\frac{dv}{dx} - \frac{dw}{dy}\right)\right.$$
$$\left. - B'\left(\frac{dv'}{dx} - \frac{du'}{dy}\right)\right\}\frac{dv}{dt} + 2(B - B')\left\{\frac{du}{dy}\frac{dw}{dt} - \frac{du}{dt}\frac{dv}{dy}\right.$$
$$\left. + \frac{du}{dz}\frac{dw}{dt} - \frac{du}{dt}\frac{dw}{dz}\right\}.$$

The coefficient of $2(B - B')$ vanishes whenever $u$, $v$, $w$ are functions of the same function of $x$, $y$, $z$ and $t$: this is the case in the problem before us.

Therefore the condition that the work should vanish is

$$\left\{B\left(\frac{dw}{dx} - \frac{du}{dz}\right) - B'\left(\frac{dw'}{dx} - \frac{du'}{dz}\right)\right\}\frac{dw}{dt} + \left\{B\left(\frac{dv}{dx} - \frac{du}{dy}\right) - B'\left(\frac{dv'}{dx} - \frac{du'}{dy}\right)\right\}\frac{dv}{dt}$$
$$= 0 \;\ldots\ldots\ldots\; (2).$$

This, with the three equations

$$u = u',\; v = v',\; w = w' \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\; (3),$$

gives us four surface conditions to determine the intensities of the reflected and refracted waves, and the directions of vibration in the wave-fronts.

Let $l, m, n$; $l_1, m_1, n_1$; $l', m', n'$ be the direction cosines of the normals to the incident reflected and refracted waves respectively.

$\alpha, \beta, \gamma$; $\alpha_1, \beta_1, \gamma_1$; $\alpha', \beta', \gamma'$ the direction cosines of the directions of vibration in these waves, $\kappa$, $\kappa_1$, $\kappa'$ the amplitudes, $V$, $V'$ the velocities in the two media.

Then we may represent the disturbances by

$$u = \alpha\kappa f\left(\frac{lx + my + nz}{V} - t\right)$$

$$+ \alpha_1\kappa_1 f\left(\frac{l_1x + m_1y + n_1z}{V} - t\right),$$

$$u' = \alpha'\kappa' f\left(\frac{l'x + m'y + n'z}{V'} - t\right).$$

The equation $u = u'$ when $x = 0$ gives

$$\alpha\kappa f\left(\frac{my + nz}{V} - t\right) + \alpha_1\kappa_1 f\left(\frac{m_1y + n_1z}{V} - t\right)$$

$$= \alpha'\kappa' f\left(\frac{m'y + n'z}{V'} - t\right).$$

Whence

$$\frac{m}{V} = \frac{m_1}{V} = \frac{m'}{V'},$$

$$\frac{n}{V} = \frac{n_1}{V} = \frac{n'}{V'},$$

and

Similarly

$$\left.\begin{aligned} \alpha\kappa + \alpha_1\kappa_1 &= \alpha'\kappa'. \\ \beta\kappa + \beta_1\kappa_1 &= \beta'\kappa' \\ \gamma\kappa + \gamma_1\kappa_1 &= \gamma'\kappa' \end{aligned}\right\} \qquad \text{(4)}.$$

And equation (2) gives,

since $$\frac{dw}{dt} = \frac{dw'}{dt}, \quad \frac{dv}{dt} = \frac{dv'}{dt},$$

$$\gamma'\kappa'\left\{\frac{B}{V}(l\gamma - n\alpha)\kappa + (l_1\gamma_1 - m_1\alpha_1\kappa_1) - \frac{B'}{V'}(l'\gamma' - n'\alpha'\kappa')\right\}$$

$$+ \beta'\kappa'\left\{\frac{B}{V}(l\beta - m\alpha)\kappa + (l_1\beta_1 - m_1\alpha_1\kappa_1) - \frac{B'}{V'}(l'\beta' - m'\alpha'\kappa')\right\}.$$

Now let the intersection of the incident wave-front and the plane $x = 0$ be taken as axis of $y$,

$$m = 0, \text{ therefore } m_1 = m' = 0.$$

Let $\phi$ be the angle which the incident wave normal produced makes with the positive part of the axis of $x$.

Let $\theta$ be the angle which the direction of vibration makes with the axis of $y$.

Let $\phi_1$, $\theta_1$, $\phi'$, $\theta'$ have similar meanings for the other two waves, then we have

$$l = \cos\phi,\ m = 0,\ n = \sin\phi,$$

$$\alpha = \sin\phi\sin\theta,\ \beta = \cos\theta,\ \lambda = -\cos\phi\sin\theta,$$

with similar equations for the others.

Also the equations

$$\frac{n}{V} = \frac{n_1}{V} = \frac{n'}{V'}$$

become
$$\frac{\sin\phi}{V} = \frac{\sin\phi_1}{V} = \frac{\sin\phi'}{V'};$$

therefore
$$\sin\phi = \sin\phi_1,$$
$$\phi_1 = \pi - \phi,$$

and the surface conditions reduce to

$$\kappa\sin\phi\sin\theta + \kappa_1\sin\phi\sin\theta_1 = \kappa'\sin\phi'\sin\theta' \;\ldots\ldots\; (5),$$

$$\kappa\cos\theta + \kappa_1\cos\theta_1 = \kappa'\cos\theta' \;\ldots\ldots\; (6),$$

$$-\kappa\cos\phi\sin\theta + \kappa_1\cos\phi\sin\theta_1 = -\kappa'\cos\phi'\sin\theta' \;\ldots\; (7),$$

$$\kappa'\cos\phi'\sin\theta'\left[\frac{B}{V}(\kappa\sin\theta + \kappa_1\sin\theta_1) - \frac{B'}{V'}\kappa'\sin\theta'\right]$$
$$+\kappa'\cos\theta'\left[\frac{B}{V}(\kappa\cos\phi\cos\theta - \kappa_1\cos\phi\cos\theta_1)\right.$$
$$\left.-\frac{B'}{V'}\kappa'\cos\phi'\cos\theta'\right] = 0 \;\ldots\ldots\; (8).$$

From (5) and (7)

$$\kappa_1\sin\theta_1 = -\kappa\sin\theta\frac{\sin(\phi-\phi')}{\sin(\phi+\phi')} \;\ldots\ldots\; (9),$$

$$\kappa'\sin\theta' = \kappa\sin\theta\frac{\sin 2\phi}{\sin(\phi+\phi')} \;\ldots\ldots\; (10).$$

Now
$$B = V^2\rho,\quad \frac{B}{V} = V\rho = h\rho\sin\phi,$$

if
$$h = \frac{V}{\sin\phi} = \frac{V'}{\sin\phi'},$$

$$\frac{B'}{V'} = h\rho' \sin\phi'.$$

And we get from (8)

$$\cos\phi' \sin\theta' \left[\rho \sin\phi \left(\kappa \sin\theta + \kappa_1 \sin\theta_1\right) - \rho' \sin\phi' \left(\kappa' \sin\theta'\right)\right]$$
$$+ \cos\theta' \left[\rho \sin\phi \cos\phi \left\{\kappa \cos\theta - \kappa_1 \cos\theta_1\right\}\right.$$
$$\left. - \rho' \sin\phi' \cos\phi' \kappa' \cos\theta'\right] = 0 \;\ldots\ldots\ldots\; (11).$$

If we put $\theta = \frac{\pi}{2}$, *i.e.*, consider only vibrations in the plane of incidence, then it is clear, from symmetry, we must have

$$\theta' = \theta_1 = \frac{\pi}{2}.$$

Hence from (11)

$$\rho\left(\kappa + \kappa_1\right) \sin\phi = \rho' \kappa' \sin\phi'.$$

But from (5)

$$\left(\kappa + \kappa_1\right) \sin\phi = \kappa' \sin\phi',$$

therefore
$$\rho = \rho' \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\; (12),$$

or the density is the same throughout the two media and (11) reduces to

$$\sin 2\phi \left(\kappa \cos\theta - \kappa_1 \cos\theta_1\right) = \sin 2\phi' \kappa' \cos\theta'.$$

But
$$\kappa \cos\theta + \kappa_1 \cos\theta_1 = \kappa' \cos\theta'.$$

Hence
$$\kappa_1 \cos\theta_1 = \kappa \cos\theta \frac{\sin 2\phi - \sin 2\phi'}{\sin 2\phi + \sin 2\phi'},$$

$$= \kappa \cos\theta \frac{\tan\left(\phi - \phi'\right)}{\tan\left(\phi + \phi'\right)} \;\ldots\ldots\ldots\ldots\ldots\ldots\; (13),$$

$$\kappa' \cos\theta' = \kappa \cos\theta \left\{1 + \frac{\tan\left(\phi - \phi'\right)}{\tan\left(\phi + \phi'\right)}\right\},$$

$$= \kappa \cos\theta \frac{\sin 2\phi}{\sin\left(\phi + \phi'\right) \cos\left(\phi - \phi'\right)} \;\ldots\; (14).$$

The four equations (9), (10), (13), (14) give the amplitudes and directions of vibration for the reflected and refracted waves, and have been found by MacCullagh (*Irish Transactions*, 1848), assuming a form for the potential energy of the strained medium which, as Stokes has shewn, is inconsistent with the Conservation of Energy. It is perhaps worth remarking that if we denote by $V$

the potential energy of the medium by $M$, the form assumed for it by MacCullagh, then we can put $V = M + W$; and if the vibrations be transverse $W$ does not occur in the equations either of motion or of condition at the surface, but is zero if $u, v, w$ are functions of the same function of $x, y, z$ and $t$.

MacCullagh assumes *ab initio* that $\rho = \rho'$, which follows of necessity from equations 5 and 8.

I have thought it interesting to shew that these results can be deduced from the correct expression for $V$, assuming the density of the ether the same in the two media. The total intensity of the reflected ray will be the same as that given by Fresnel's expressions, that of the refracted ray will differ from his.

The amplitude of the vibration in the plane of incidence given above is the same as that for the vibration perpendicular to the plane of incidence on Fresnel's theory, and *vice versâ*.

Moreover, we have from 9 and 13

$$\tan\theta_1 = -\tan\theta \frac{\cos(\phi - \phi')}{\cos(\phi + \phi')},$$

$\theta$, $\theta_1$ being the angles which the directions of vibration make with the intersection of the wave-fronts and the reflecting surface.

If $\alpha$, $\beta$ are the angles between the plane of incidence and the directions of vibration

$$\alpha = \frac{\pi}{2} - \theta, \quad \beta = \frac{\pi}{2} - \theta_1,$$

$$\tan\beta = -\tan\alpha \frac{\cos(\phi + \phi')}{\cos(\phi - \phi')} \quad \ldots\ldots\ldots\ldots\ldots(15).$$

And if further we suppose that the directions of vibration lie in the planes of polarization, this is the formula which has been verified by Fresnel and Brewster.

For the refracted wave we have

$$\tan\theta' = \tan\theta\cos(\phi - \phi');$$

$$\therefore \tan\beta' = \frac{\tan\alpha}{\cos(\phi - \phi')} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots(16);$$

this also agrees with Fresnel's formulæ.

Again, Green has shewn that the change of phase arising from total reflexion can be deduced from his formulæ, and the expressions he arrives at agree with Fresnel's and are the foundation of the theory of Fresnel's rhomb. I propose to shew that the

same expressions can be deduced without his assumption that $B = B'$.

Let us first take the case in which the vibrations lie in the plane of incidence, then

$$v = v' = 0,$$

and $w$ and $u$ are independent of $y$.

The equations of motion are

$$\rho \frac{d^2u}{dt^2} = B \frac{d}{dz}\left(\frac{du}{dz} - \frac{dw}{dx}\right),$$

$$\rho \frac{d^2w}{dt^2} = B \frac{d}{dw}\left(\frac{dw}{dx} - \frac{du}{dz}\right),$$

with similar equations for the other medium.

The conditions at the surface become

$$u = u', \quad w = w'.$$

Assume with Green

$$u = \frac{d\psi}{dz}, \quad w = -\frac{d\psi}{dx}.$$

Then, for the equation of motion, we have

$$\frac{d^2\psi}{dt^2} = V^2\left(\frac{d^2\psi}{dz^2} + \frac{d^2\psi}{dx^2}\right),$$

if $V^2 = \dfrac{B}{\rho}$.

And for the second medium

$$\frac{d^2\psi'}{dt^2} = V'^2\left(\frac{d^2\psi}{dz^2} + \frac{d^2\psi}{dx^2}\right).$$

The equations are satisfied by

$$\psi = f\left(\frac{lx + nz}{V} - t\right) + f\left(\frac{l_1x + n_1z}{V_1} - t\right),$$

$$\psi' = f'\left(\frac{l'x + n'z}{V'} - t\right),$$

with the condition

$$\frac{n}{V} = \frac{n_1}{V_1} = \frac{n'}{V'};$$

therefore $$n' = \frac{V'n}{V},$$

and if $V'$ is $> V$, $n'$ may be greater than unity, and therefore $l'$ becomes imaginary.

In this case, assuming the functions $f$, $f_1$ to be harmonic in form, the equation is satisfied by

$$\psi = \kappa' e^{-\frac{l'}{V'}x} \sin(hz - t + e'),$$

where

$$h = \frac{n'}{V'} = \frac{n}{V},$$

$$l'^2 = n'^2 - 1$$

$$= \left(\frac{n^2 V'^2}{V'} - 1\right)$$

$$= (\mu^2 n^2 - 1) \text{ ........................(17).}$$

And for the first medium we have

$$\psi = \kappa \sin\left(\frac{lx}{V} + hz - t\right) + \kappa_1 \sin\left(\frac{l_1 x}{V_1} + hz - t + e_1\right).$$

It is necessary to introduce the quantity $e_1$, for as with Green one of the conditions introduces cosines of $hz - t$ on the right hand, the other sines, so that it is impossible to satisfy both unless we have both cosines and sines on the left, put $hz - t = \chi_0$, then the surface conditions give, since

$$l = -l_1 = \cos\phi, \quad V_1 = V,$$

$$\kappa \cos\chi_0 + \kappa_1 \cos(\chi_0 + e_1) = \kappa' \cos(\chi_0 + e') \text{.........(18),}$$

$$\frac{\cos\phi}{V}\{\kappa\cos\chi_0 - \kappa_1\cos(\chi_0 + e_1)\} = -\frac{\kappa' l'}{V'}\sin(\chi_0 + e') \text{.............(19).}$$

From (19), since $\frac{V'}{V} = \mu$,

$$\mu\cos\phi\{\kappa\cos\chi_0 - \kappa_1\cos(\chi_0 + e_1)\}$$
$$= -\kappa'\sqrt{(\mu^2\sin^2\phi - 1)}\sin(\chi_0 + e').$$

Hence, expanding and equating coefficients of $\cos\chi_0$ and $\sin\chi_0$,

$$\kappa + \kappa_1\cos e_1 = \kappa'\cos e',$$

$$\kappa_1 \sin e_1 = \kappa' \sin e',$$

$$\mu\cos\phi\{\kappa - \kappa_1\cos e_1\} = -\kappa'\sqrt{(\mu^2\sin^2\phi - 1)}\sin e',$$

$$\mu\cos\phi\kappa_1\sin e_1 = -\kappa'\sqrt{(\mu^2\sin^2\phi - 1)}\cos e'.$$

Substitute for $\kappa'\sin e'$. $\kappa'\cos e'$ in form.

Solving these equations, we get

$$\kappa_1 \cos e_1 = \kappa \frac{\mu^2 \cos 2\phi + 1}{\mu^2 - 1} \quad\text{..................(20)},$$

$$\kappa_1 \sin e_1 = -\kappa \frac{2\mu \cos\phi \sqrt{(\mu^2 \sin^2\phi - 1)}}{\mu^2 - 1} \quad\text{...........(21)},$$

$$e_1 = -\tan^{-1} \frac{2\mu \cos\phi \sqrt{(\mu^2 \sin^2\phi - 1)}}{\mu^2 \cos 2\phi + 1} \quad\text{...........(22)}.$$

Green's formulæ for light polarized in the plane of incidence is

$$e_1 = 2 \tan^{-1} \frac{\sqrt{(\mu^2 \sin^2\phi - 1)}}{\mu \cos\phi},$$

and this agrees with the result above.

Also squaring and adding (20) and (21)

$$\kappa_1 = \kappa,$$

or the amplitude of the reflected wave equals that of the incident.

Now let the vibrations be perpendicular to the plane of incidence.

The equations are

$$\frac{d^2v}{dt^2} = V^2 \left(\frac{d^2v}{dx^2} + \frac{d^2v}{dz^2}\right),$$

$$\frac{d^2v'}{dt^2} = V'^2 \left(\frac{d^2v'}{dx^2} + \frac{d^2v'}{dz^2}\right),$$

$$u = u' = w = w' = 0,$$

$$v = v',$$

$$B \frac{dv}{dx} = B' \frac{dv'}{dx},$$

and the solution as before can be written

$$v = \kappa \sin\left(\frac{lx}{V} + hz - t\right)$$

$$+ \kappa_1 \sin\left(-\frac{lx}{V} + hz - t + e_1\right),$$

$$v' = \kappa' e^{-\frac{l'x}{V'}} \sin(hz - t + e')$$

where
$$l'^2 = \mu^2 n^2 - 1$$
$$= \mu^2 \sin^2\phi - 1.$$

The surface conditions give

$$\kappa \sin \chi_0 + \kappa_1 \sin (\chi_0 + e_1) = \kappa' \sin (\chi_0 + e') \quad\text{..........(23)},$$

$$\frac{l}{V}\{\kappa \cos \chi_0 - \kappa_1 \cos (\chi_0 + e_1)\}$$

$$= -\frac{B'}{B}\kappa' \frac{l'}{V'} \sin (\chi_0 + e')\,;$$

hence, since

$$\rho = \rho' \text{ and } \frac{B'}{B} = \mu^2,$$

$$\cos \phi \{\kappa \cos \chi_0 - \kappa_1 \cos (\chi_0 + e_1)\}$$

$$= -\kappa'\mu \sqrt{\{\mu^2 \sin^2 \phi - 1\}} \sin (\chi_0 + e') \quad\text{............(24)}.$$

Thus

$$\kappa + \kappa_1 \cos e_1 = \kappa' \cos e'.$$

$$\kappa_1 \sin e_1 = \kappa' \sin e',$$

$$\cos \phi (\kappa - \kappa_1 \cos e_1)$$

$$= -\kappa'\mu \sqrt{(\mu^2 \sin^2 \phi - 1)} \sin e',$$

$$\kappa_1 \cos \phi \sin e_1 = -\kappa'\mu \sqrt{(\mu^2 \sin^2 \phi - 1)} \cos e'.$$

Solving

$$\kappa_1 \cos e_1 = \kappa \frac{\cos^2 \phi + \mu^2 - \mu^4 \sin^2 \phi}{\cos^2 \phi - \mu^2 + \mu^4 \sin^2 \phi} \quad\text{.................(25)},$$

$$\kappa_1 \sin e_1 = -\frac{2\mu \sqrt{(\mu^2 \sin^2 \phi - 1)} \cos \phi}{\cos^2 \phi - \mu^2 + \mu^4 \sin^2 \phi} \quad\text{.............(26)},$$

$$\tan e_1 = -\frac{2\mu \cos \phi \sqrt{(\mu^2 \sin^2 \phi - 1)}}{\cos^2 \phi + \mu^2 - \mu^4 \sin^2 \phi} \quad\text{...........(27)},$$

whence

$$\tan \tfrac{1}{2} e_1 = -\frac{\mu}{\cos \phi} \sqrt{(\mu^2 \sin^2 \phi - 1)} \quad\text{................(28)}.$$

This agrees with Fresnel's formulæ for light polarized perpendicular to the plane of reflexion.

And as before $\kappa_1 = \kappa$, or all the energy in the incident wave has been transferred to the reflected.

Thus if we have a wave of polarized light incident so as to be totally reflected and resolve it into two waves polarized in and perpendicular to the plane of incidence, the difference of phase in the two reflected waves is exactly that given by Fresnel, and the theory on which he based his rhomb still holds.

It is perhaps worth notice that the result as to the change of phase for the vibrations in the plane of incidence is independent of the equation $\rho = \rho'$.

Since the above was written, I have found that Ketteler has given in Wiedemann's *Annalen*, Vol. III., Part 1, 1878, equations from which the change of phase which accompanies total reflexion may be deduced.

*February* 23, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

Lord Rayleigh, M.A., Professor of Experimental Physics, William Burnside, M.A., Fellow of Pembroke College, John Greaves, B.A., Fellow of Christ's College, and A. J. C. Allen, B.A., Fellow of Peterhouse, were ballotted for and duly elected Fellows of the Society.

Professor Willie Kuhne, of Heidelberg, formerly of Amsterdam, Adolf Erik Nordenskiöld, Commander of the Swedish Arctic expeditions of 1864, 1868, 1872—3, 1878—9, and William Henry Flower, F.R.S., Hunterian Professor in the Royal College of Surgeons of England, having been nominated by the Council, were ballotted for and duly elected Honorary Members of the Society.

The following communications were made to the Society:—

(1) Professor T. McK. HUGHES, M.A., *On the transport of fine mud and vegetable matter by conferva.*

It has, I dare say, been observed that often in warm summer weather rivers, like the Cam above Cambridge for instance, or the Lea near Hertford, run quite turbid, though generally bright and clear. If we examine the bottom of such a stream we may notice patches of velvety growth on the mud, and when the sun shines, the oxygen given off by these plants is seen in sparkling bubbles tangled in the fibres. If we watch, we see one mass after another slowly disengage itself from the mud and rise to the surface of the water, sometimes coming up edgewise when the little gas-floats are more numerous on one side than another. Gas, probably carburetted hydrogen, sometimes appears to be let out from the mud below on the removal of the covering mat of conferva. The fine mud lifted with the root end of the plant leaves it gradually as it ascends, and when the process has been going on for some time, the water appears so muddy that it might be supposed that cattle or ducks or men had been disturbing it higher up the stream.

The masses of conferva float down the stream till the gas bubbles have escaped, then sink again, and so in bright rainless weather the work of denudation and the sea-ward transport of solid matter goes on.

Another curious phenomenon of a similar kind may be observed in the autumn in the meres of Shropshire, where it is known as the '*Breaking of the Water.*' The water first assumes a brownish tint which becomes more yellow, then more green. The green matter then rises and forms a scum on the surface. Some of this is blown by the wind and stranded on the shore or caught among the reeds along the margin of the lake. The rest sinks to the bottom and disappears. The water then becomes perfectly clear again.

During the earlier stages the water gives off a very offensive smell, and is quite unfit for household purposes. Mr Salusbury Mainwaring, of Oteley, to whom I am indebted for much of the information I have collected on the subject, informed me that the phenomenon occurs regularly every autumn in Ellesmere, so that arrangements have to be made with a view to supplying the house with water from other sources for the few weeks during which the water of the mere is breaking.

I collected a quantity of the green filmy mass that had gathered along the leeward margin of Ellesmere last September, and gave some of it to Mr Shepheard of the Chester Natural Science Society. He examined it under the microscope, and found that it consisted of a mass of small faggots of a very minute freshwater alga, and thought that on the whole it most resembled *Conferva echinulata* described and figured by Sowerby (*Eng. Bot.* Ed. 1805, p. and tab. 1378).

The phenomenon is mentioned in *Mountain, Meadow and Mere*, p. 16, by G. C. Davies, and more fully described by the Rev. W. A. Leighton in the Report of the Severn Valley Naturalists Field Club, published in the *Midland Naturalist*, Vol. I. 1878, p. 258. The explanation of the changes in the water would therefore seem to be that first the alga lies in the fine mud at the bottom till the period of reproduction in August and September, when, expanding, it rises towards the surface, itself a dark object, and carrying with it some mud; and hence the brown and yellow colours.

Then some of the old plant suffers decomposition and also perhaps a little carburetted hydrogen is disengaged from the mud below; hence the offensive smell.

Next, a bright green filamentous growth is developed; the plant breaks up, and that part which is to hibernate till the same process is repeated in the following autumn now sinks to the bottom and the rest perishes on the shore or in the water.

As the plant during its period of hibernation lies at the bottom and some of the fine sediment which is carried by the rains into the lake must settle on it, it seems hardly possible that it could disengage itself from the mud in the summer without lifting some with it from the bottom, though it must clearly be only a very small quantity of the very finest. This point however I have not yet been able to verify by obtaining some of the water when the plant was beginning to rise, and it cannot be observed in the same way as in the case of the coarser confervoid growths.

But it seems most probable that by this also among the small but ceaseless operations of nature the mud is being unsettled, lifted and drifted now further out, now nearer shore, to be left at rest only when it has dropped into water too deep, or on a spot otherwise unsuitable for the plants which help to transport it.

(2) Professor T. McK. HUGHES. *On the altered rocks of Anglesea.*

In 1820, Professor Henslow made a careful collection of the rocks of the island and gave a description of them and their mode of occurrence in an excellent paper published in the *Transactions* of this Society[1]. This collection is in the Woodwardian Museum.

Professor Sedgwick worked over the district, but has only referred to it here and there in his published Memoirs. From his MS. notes, however, we learn that he generally agreed with Henslow's identifications.

Professor Ramsay has described the district, in detail in the Maps[2], Sections[3], and Memoir[4] of the Geological Survey. I find myself obliged to return much nearer to his views in some cases where the opinions of subsequent writers[5] had prepared me for a different interpretation, and I would refer the principal schistose masses to altered Cambrian and Silurian, though to a different part of the series from that to which he would assign it, and though I generally offer a different explanation of the nature of the change that has taken place.

The definition of metamorphism is beset with difficulties, for few rocks have retained the character they had when first formed, and the most extreme alteration and replacement does not seem to be connected with the greatest changes of temperature as inferred from other evidence.

It is quite common in examining folded rocks to find that a shale has been rolled out as it were at the expense of its thickness, so as in its undulations or crumples to cover a much larger superficial extent than it did originally. One proof of this is that when

[1] *Trans. Phil. Soc.* 1821.
[2] Sheets 75, 78.
[3] Sheet 40.
[4] *Mem. Geol. Surv.* Vol. III.
[5] Hicks, *Q. J. G. S.* Vol. XXXV. p. 295. Callaway, *Q. J. G. S.* Vol. XXXVI. p. 2.

we have subordinate hard unyielding beds which are incapable of this kind of extension, they are broken up and discontinuous, and often in the folds protruded through the layers of yielding shale.

Of this kind of action some interesting examples have come under my notice in the high ground between the head waters of Dent and Ribblesdale in Yorkshire. The mass of the mountain consists of Yoredale Rocks made up of shales, sandstones, and limestones. One of the limestones rests upon a thick bed of shale, and is quarried for black marble in a small ravine near the bottom of the hill. When the workmen, removing layer after layer, reach the lowest beds, the pressure of the mass on either side squeezes out the shale and the limestone slabs in the middle bulge up, and if struck with a pick along the lines of tension are apt to break and pieces fly up with considerable force. In making a tunnel on the Settle and Carlisle Railway through the upper part of the same series a similar thing was observed when too thin a floor of rock was left resting upon shale. The common phenomena of creeps in coal mines, and of the bulging up of the fore-shore when there has been a land slip, or the rising of the surrounding peat when a bank has been thrown across a moss are examples of the same kind.

This must go on among the larger rock masses also, compressed in one place or in one direction they give way in another, exhibiting in a large way what is seen on a small scale in the examples given above, and so we observe that, in consequence of large portions of our older formations being made up of compressible mud which behaves more or less as a fluid, when the great weight of ever accumulating strata is heaped up over one part of it, the pressure is transmitted laterally, and the rocks yield; compressed horizontally, they often rise elsewhere vertically.

The readjustment takes place in three ways: either (1) the rocks are thrown into great folds as is generally the case when they are composed of large beds of varying texture; or (2) they are puckered into numerous small contortions forming a gnarled or crumpled schist. This is chiefly the case when the rock is composed of laminæ of fine mud with thin layers of coarser unyielding material, as in the great masses of greenish schists in Anglesea. Or (3) all the particles are squeezed together so that those which had, or could assume, a flat or elongated form, are forced to arrange themselves with their longer diameters in planes at right angles to the lateral pressure. This happens in all masses of homogeneous mud and produces cleavage, and of this we have plenty of examples in the black slates of Anglesea. All these are only different results of the same force, and from the nature of the case it must be a local phenomenon depending upon

the character and arrangement of the materials of which the rocks are composed and not likely to be of wide extent under any probable conditions; so that it is no argument against the correlation of rocks differing in this respect that if contemporaneous they should be similarly affected. We observe also all combinations of the results above described; sometimes the rock is cleaved between two beds that would not cleave; sometimes the rock is crumpled between two even-bedded masses, and generally the long folds of the puckered rock more or less coincide with cleavage planes in parts of the mass.

Accompanying these phenomena are others of great importance in considering the question of the nature of the changes which have affected the Anglesea schists. Where in the folded rocks we find beds of hard unyielding sandstone or limestone they are often crushed and veined in all directions to such an extent sometimes, that the whole mass becomes what, if we saw it in lines and strings, we should call vein stuff.

Obviously, we are not referring to ordinary precipitation of layers of various minerals on the walls of a gaping fissure. We are considering the cases in which the rocks are suffering from compression and no open cavities exist. We find the whole mass divided into angular portions retaining the original texture and colour, and the dividing parts in every stage of alteration till granular quartzite has passed into white or semi-transparent quartz, and all the colour has been discharged. This may be well seen in some of the grits near Garth ferry and on the coast north of Dulas Bay, which are subordinate to the schists we are considering.

The quantity of colouring matter in such a grit is very small and is probably chiefly a silicate of iron coating the grains. As the process goes on, impurities are left out along the divisional planes between the areas of crystallization first forming those coloured films which occur along the smaller joint surfaces and eventually carried away by the percolating water. So in the limestones such as those seen between Amlwch and Llanpadrig, it is a kind of indigenous vein structure which we first see in the crushed rock. This we follow into the mass till none of the rock exhibits its original character and the whole is a crystalline limestone often more or less siliceous. Silica is apt to replace carbonate of lime under such circumstances as *e.g.* the tabular flint lining cracks and joints in the chalk; the chert irregularly replacing large masses of carboniferous limestone, and, I believe, the cherty jaspery beds we often find in the Silurian and Cambrian, where, from what we find in other undisturbed districts, we should expect calcareous deposits. The flinty beds of the coast near Glanffynon may perhaps be thus explained.

The anthracitic bands and films in some of the older rocks probably represent the bituminous colouring of limestones and other rocks got rid of during the process of crystallization, and left along joints or clayey bands coinciding with the bedding.

When on a small scale nodules, or on a large scale masses, of hard rock lying in compressible shale are subjected to contortion, the shale is squeezed out over the harder masses producing a kind of fault all round between the harder and softer rock, and giving rise to slickensides and similar phenomena, and often mineral changes are set up along the parts thus more crushed.

In the case of a crumpled and gnarled shale in which there are thin laminæ of harder and softer beds, this unequal yielding must produce similar results and a kind of slickensides must pervade the whole mass. The beds will readily split along the lines of shale exhibiting shiny slickensided faces, especially if there be a good deal of mica, or talc, or other magnesian minerals in the shale. A minute veiny structure will be very common in the more crushed portions.

Hard rocks crushed up and veined will be apt to become serpentinous, and if it happens to be a limestone probably masses of Ophicalcite will represent it.

Such I take it is the origin of the mica schists and associated rocks in the four great masses of Anglesea. They are simply mechanically altered shales with subordinate hard rocks here and there, and were probably derived largely from volcanic material. They consist of finer and coarser laminæ of more or less siliceous felspathic mud, and are in nowise to be regarded as true mica schists.

There are two other kinds of mica rock of very different origin.

1. A rock made up almost entirely of mica fragments such as may be found associated with other granite debris. For on the destruction of a granite we have either an arkose, where all the various ingredients are again thrown together so that the sedimentary rock derived from the granite resembles it in composition—or we may have the quartz and perhaps the felspar carried to one place, and the mica transported a little further and allowed to settle by itself. Probably we have some beds of this kind between Amlwch and Dulas.

2. There are rocks in which there has been an entire rearrangement of the component minerals by chemical action, different from that referred to above under the head of veining. In that a mineral was taken away or a mineral was introduced along definite lines by percolating water carrying with it something which had an affinity for the transported mineral or its solvent, but the main mass remained passive, received these gradual

changes from without, and we generally get a simpler rock as the result. In the case we are now considering, the whole mass is started off on a kind of mutual exchange system and the materials already there are resorted, giving as the result segregations of complex minerals such as occur in gneiss, &c. In these we get mica schists when the mica has come into prominence by crystallization after the deposition of the material of which the rock is composed. This is the only rock to which the term mica schist should be applied. A sedimentary mica rock splitting along bedding planes is a shale. A sedimentary mica rock splitting along cleavage planes is a slate. But a mica rock which we have reason to believe owes its present character to metamorphism, *i.e.* to a mineral rearrangement of the whole mass, and which splits along the mica plates, but in which we cannot say whether they coincide in their arrangement with bedding or cleavage, may be properly called a mica schist.

We will now apply these observations in greater detail to the explanation of the Anglesea rocks, omitting for the present all criticism of the boundary lines, which with a new reading of the rocks would have to be somewhat modified. It will be seen on reference to the survey map[1] that there is a central axis of granitic rocks represented as running approximately N.E. and S.W. from near Llanfaelog to near Llanfihangeltre'rbeirdd. This consists of mica schists, hornblende schists, gneiss, and a granitoid rock made up of a crystalline aggregate of quartz, orthoclase felspar and less commonly mica. It is often succeeded by a grey felsitoid rock for which some, till we know all about its origin, prefer to use another vague term borrowed from the Swedes, calling it Hälleflinta. The felsitic rocks seem to overlie the granitic, but there are passage beds of varying thickness between them. I think there is sufficient evidence to show that these felsitic rocks are sometimes metamorphosed into the granitoid rock. There are rapid alternations of the two kinds of rock and one shades off into the other in a manner which shows that it is not difference of composition, or different rates of cooling, or anything but a slow segregation of the quartz, felspar, and other minerals that has produced the change. Similar examples were pointed out by Dr Callaway in the *Wrekin*, where the change seems to have taken place all round a mass from the joint surfaces so that a block of hälleflinta has a granitoid outside. We can attribute such change to chemical action only, and the process that would best explain the phenomena observed seems to me to be somewhat like that described by Prof. Liveing, *Proc. Camb. Phil. Soc.* Vol. III. p. 75.

Further, it will be seen that there are four great masses of schist coloured pink on the map: the first lying along the N. shore

[1] Sheet 78.

of the island around and W. of Amlwch: the second on the W. in and stretching away to the N.E. of Holyhead Island: the third S.E. of the central axis as far as Llangefni: and the fourth touching the Menai Straits.

Now I would first call attention to this point, that the schists of the central mass are quite different from those which cover the four great areas above defined. There are mica schists as well as hornblende schists and gneiss in the central mass. But most of it seems a truly metamorphic rock in which the minerals have been rearranged, and in which the mica was not deposited as mica, not at any rate in the form and condition in which we find it now. The great masses of the so-called schists of Anglesea away from the central axis I take to be hardly metamorphosed at all, but to be simply gnarled and crumpled rocks which consist principally of thin laminated sandstones and shales, and in which the glossy appearance is, in almost all cases, due to a kind of slickensides pervading the whole mass as the harder sandy layers were crumpled in the yielding shale[1].

If then the schists of Amlwch, Holyhead, Llangefni, and Menai do not belong to the old truly metamorphic series, what is their place? To work this out the first and most important question is, what is their relation to the unaltered black slates and shale?

It is very rarely that a junction between them and the underlying series can be seen in section, and it is true that where seen nearly in contact in the little creek of Porth y corwg there is a crack which would be in the position of the fault supposed by the Survey to throw the shales against the gnarled series on the supposition that the gnarled shales were altered Harlech Beds. But it did not appear to me that there was any great displacement there, and moreover it leaves a portion of the gnarled series on the S. side of the principal crack. There is a mass fallen forward from the cliff which gives the section the appearance of being more disturbed by faults than it is. Besides the improbability of there being a great curved fault running so far exactly along the junction of the black slates and gnarled beds, we can hardly allow the possibility in this case of such an enormous downthrow as would be necessary to bring the slaty series from above the horizon of the Llanpadrig quartzites and limestones down to a low part of the gnarled series. And there is nowhere else for them to be thrown from, for the Cambrian is seen in section along the flanks of the central axis, that is to say we have Arenig, Tremadoc and (Lingula Flags being so far unrecognized) we have next below the Tremadoc the basement Cambrian conglomerates resting upon the Precambrian gneiss. There is nothing that could be metamorphosed into the gnarled schists. So it seems to me better to accept the more

[1] The specimens exhibited in illustration are in the Woodwardian Museum.

obvious interpretation and admit that this slatey series does pass, as it everywhere appears to dip, under the gnarled series.

Next, what evidence can be obtained from the relation of the gnarled series to the overlying beds?

If we cross the Anglesea schists near Amlwch, we find among the highest beds, limestones, conglomerates and quartzites with black slates dropped in by faults here and there near Llanpadrig, but whether we have in these the lower May Hill or Upper Bala or something older still there is as yet no fossil evidence to show.

Succeeding the schists of the Menai Straits on the E. we have the strip of black slates and shales running from near Beaumaris N. to the sea near Glanffynon. These are supposed by Prof. Ramsay to be the continuation of the black slates seen S.E. of the Bangor-Carnarvon ridge (*i.e.* Arenig), the strike of which would certainly carry them into this position in Anglesea. But we must remember the proved faults which have entirely interrupted the continuity of the strata across the Menai Straits. Moreover though fossils are scarce in these beds, I detected in the basement beds of the upper slate series at Glanffynon some Encrinite stems and a large Orthis about the size and outline of *O. alternata*, Sow., and though these fossils are too illpreserved for determination, they will hardly do for Arenig forms. So the evidence is that the beds are much higher in the series. If they turn out to be Upper Bala then the gnarled schists must be Lower Bala beds, or the Upper Bala must be transgressive over the Lower Bala beds and Arenig, an overlap which we have no right to assume. If they turn out to belong to the Lower May Hill group, they may rest unconformably on the gnarled schists but that they can be Arenig seems to me most improbable.

The Coniston limestone and shale rest upon the Green Slates and Porphyry in the Lake District, and on their equivalents in Chapel-le-Dale, showing that the Green Slates and Porphyry come between the Upper Bala and the Skiddaw or Arenig beds.

So the volcanic series of Snowdon is packed in between the Arenig and the Bala limestone and shale, and has here and there sedimentary deposits with fossils of Lower Bala age.

Unless we assume a number of unproved faults and allow an improbable downthrow for those that are seen, the simple explanation of the structure of Anglesea seems to be that the gnarled schists rest upon a great series of black slates in which Arenig fossils have been found and are overlaid by other black slates which the meagre fossil evidence yet obtained refers to beds not older than the Bala.

In the upper beds of the lower black slate series S. of Amlwch and Llaneilian, we have felspathic breccias, ashes and felsitic rocks

suggesting volcanic material at hand. In the gnarled series itself we see an immense thickness of tolerably uniform felspathic shales and slates with coarser lines, just such material as would be formed by the settling down of volcanic ash in water. The beds are mostly obscured by crumpling, but if we select the uncrumpled portions we shall find only layers of gritty and finer felspathic matter showing all the characters of an ordinary sedimentary deposit. Having once observed this we can almost always follow it into the crumpled and gnarled portions until obliterated by vein structure. Some beds identical in every respect occur on Snowdon, and in the rocks referred to the Green Slates and Porphyry S. of Ulswater there are schists quite as much altered as anything in the gnarled series of Anglesea. The Green Slates and Porphyry represent the Bala volcanic series and closely resemble the same beds in N. Wales. But in Chapel-le-Dale about 20 miles S.E. from Shap we find in the corresponding position nothing but sedimentary deposits over 10,000 feet in thickness, showing however in their material and arrangement, a sorting of volcanic felspathic debris by water. Not a trace of life has as yet been detected in all this great mass of sediment more than two miles in thickness. The absence of fossils therefore in the still more unpromising-looking schists of Anglesea need not strike us as a difficulty in the correlation proposed—viz. that we have in the gnarled schists of Anglesea, the equivalents in time of part of the Bala volcanic series, deposited beyond the principal region of volcanic activity and that the alteration which has taken place in them is to be referred to the crumpling of a laminated rock, and a kind of indigenous vein structure following the intense crushing and not to foliation or the segregation in layers of the different component minerals, under any influence such as is usually understood by true metamorphism.

(3) Mr W. W. Cordeaux exhibited some antlers of the fallow deer (*Cervus dama*), stated to have been recently found at Newnham. A discussion took place as to the probability of the antlers having been found in the stratum from which the workmen said they had procured them.

MONDAY, *March* 8, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

A. Vinter, M.A., Gonville and Caius College, was ballotted for and duly elected a Fellow of the Society.

The following communications were made to the Society:—

(1) Professor CAYLEY: *On the Schwarzian Derivative and the Polyhedral Functions.*

(Abstract.)

The quotient $s$ of any two solutions of a linear partial differential equation of the second order

$$\frac{d^2y}{dx^2}+p\frac{dy}{dx}+qy=0,$$

is determined by a differential equation of the third order

$$\frac{\dfrac{d^3s}{dx^3}}{\dfrac{ds}{dx}}-\frac{3}{2}\left(\frac{\dfrac{d^2s}{dx^2}}{\dfrac{ds}{dx}}\right)^2=-\frac{1}{2}\left(p^2+2\frac{dp}{dx}-4q\right),$$

where the function on the left hand is what I call the Schwarzian derivative, or say this derivative is

$$\{s, x\}, =\frac{s'''}{s'}-\frac{3}{2}\left(\frac{s''}{s'}\right)^2,$$

where the accents denote differentiations in regard to the second variable $x$ of the symbol.

Writing in general $(\mathrm{a}, \mathrm{b}, \mathrm{c} \therefore)(X, Y, Z)^2$ to denote a quadric function,

$$(\mathrm{a}, \mathrm{b}, \mathrm{c}, \tfrac{1}{2}(\mathrm{a}-\mathrm{b}-\mathrm{c}), \tfrac{1}{2}(-\mathrm{a}+\mathrm{b}-\mathrm{c}), \tfrac{1}{2}(-\mathrm{a}-\mathrm{b}+\mathrm{c}))(X, Y, Z)^2,$$

then if the equation of the second order be that of the hypergeometric series, generalised by a homographic transformation upon the variable $x$, the resulting differential equation of the third order is of the form

$$\{s, x\}=\left(\mathrm{a}, \mathrm{b}, \mathrm{c} \therefore\right)\left(\frac{1}{x-a}, \frac{1}{x-b}, \frac{1}{x-c}\right)^2,$$

and, presenting themselves in connection with the algebraically integrable cases of this equation, we have rational and integral

functions of $s$, derived from the polygon, the double pyramid and the five regular solids, and which are called polyhedral functions.

The Schwarzian derivative occurs implicitly in Jacobi's differential equation of the third order for the modulus in the transformation of an elliptic function (*Fundamenta Nova*, 1829, p. 79), and in Kummer's fundamental equation for the transformation of a hypergeometric series (1836), but it was first explicitly considered and brought into notice in two memoirs of Schwarz, 1869 and 1873; the last of these (relating to the algebraic integration of the differential equation for the hypergeometric series) is the fundamental memoir upon the subject, but the theory is in some material points completed in the memoirs of Klein and Brioschi.

I propose in the present memoir to consider the whole theory and in particular to give some additional developments in regard to the polyhedral functions.

I remark that Schwarz starts with the foregoing equation

$$\{s, x\} = \left(\mathrm{a}, \mathrm{b}, \mathrm{c} \;\therefore\!\between \frac{1}{x-a}, \frac{1}{x-b}, \frac{1}{x-c}\right)^2,$$

and he shows (by very refined reasoning founded on the theory of conformable figures, and which is in part reproduced) that this equation was in fact algebraically integrable for 16 different sets of values of the coefficients a, b, c. It may I think be taken to be part of his theory, although not very clearly brought out by him, that these integrals are some of them of the form, $x =$ rational function of $s$, others of the form, rational function of $x =$ rational function of $s$, the rational functions of $s$ being in fact the same in these last as in the first set of solutions, and being quotients of polyhedral functions.

But as regards the second set of cases, the solution of these (writing for convenience a new variable $z$ in place of $x$) may be made to depend upon the solution in the form, $x =$ rational function of $z$, of an equation of a somewhat similar form, but involving two quadric functions of $x$ and $z$ respectively, viz. the equation

$$\{x, z\} + \left(\frac{dx}{dz}\right)^2 \left(\mathrm{a}, \mathrm{b}, \mathrm{c} \;\therefore\!\between \frac{1}{x-a}, \frac{1}{x-b}, \frac{1}{x-c}\right)^2$$
$$= \left(\mathrm{a}_1, \mathrm{b}_1, \mathrm{c}_1 \;\therefore\!\between \frac{1}{z-a_1}, \frac{1}{z-b_1}, \frac{1}{z-c_1}\right)^2,$$

and we have the theorem that the solution of this equation depends upon the determination of $P$, $Q$, $R$, rational and integral

functions of $z$ (containing each of them multiple factors), which are such that $P+Q+R=0$: (using accents to denote differentiation in regard to $z$ this implies $P'+Q'+R'=0$, and consequently

$$QR'-Q'R=RP'-R'P=PQ'-P'Q):$$

and are further such that the equal functions $QR'-Q'R$, $RP'-R'P$, $PQ'-P'Q$ contain only factors which are factors of $P$, $Q$ or $R$. In fact writing $f, g, h=b-c, c-a, a-b$, the required relation between $x$, $z$ is then expressed in the symmetrical form

$$f(x-a) : g(x-b) : h(x-c) = P : Q : R.$$

The last mentioned differential equation is considered by Klein and Brioschi: the solutions in 13 cases, or such of them as had not been given by Schwarz, were obtained by Brioschi: and those of the remaining three cases (subject to a correction in one of them) were afterwards obtained by Klein.

The first part of the present memoir relates, say to the foregoing equation

$$\{s, x\} = \left(\mathrm{a}, \mathrm{b}, \mathrm{c} \;\therefore\!\!\right)\!\!\left(\frac{1}{x-a}, \frac{1}{x-b}, \frac{1}{x-c}\right)^2,$$

although the other form in $\{x, z\}$ may equally well be regarded as the fundamental form: and we consider in the theory

A. The derivative $\{s, x\}$, meaning as above explained.

B. Quadric functions of any three or more inverts $\dfrac{1}{x-l}$.

C. Rational and integral functions $P$, $Q$, $R$ having a sum $=0$, and which are such that

$$QR'-Q'R=RP'-R'P=PQ'-P'Q$$

contains only the factors of $P$, $Q$, $R$.

D. The differential equation of the third order.

E. The Schwarzian theory in regard to conformable figures and the corresponding values of the imaginary variables $s$ and $x$.

F. Connection with the differential equation for the hypergeometric series.

The second part of the memoir relates to the polyhedral functions.

(2) J. B. Pearson, D.D., *Observations of the Sun on the Northern Sea-horizon, taken between* 10 *p.m. and* 3 *a.m. August* 1—2, 1879, *at and near the North Cape.*

These observations were taken with the view of ascertaining how far the observed altitude of the Sun would correspond with that required by theory. They were taken from the deck of a steamer with a prism-circle of the Pistor and Martens' type, ten inches in diameter, graduated to 20″, in good adjustment: referred to in my own *Series of Lunar Distances*, p. v. They are 32 in number, but, on account of apparent irregularity, in a few cases the computation has been made on the mean of two taken together. The first was taken close to the small harbour of Kielvig, about ten miles to the east of the North Cape, which was passed at a short distance about three quarters of an hour before the Sun crossed the northern meridian. Three or four more sights were taken before the Sun's upper limb approached the visible horizon so near that the distance could not practically be ascertained; which was about twenty-five minutes before the time of meridian passage: after that the ship was delayed at a small harbour named Gjœsvör for about an hour, so that when the first view of the rising Sun's disk was obtained, its upper limb was at an altitude of 5′ 50″: about 40 minutes after its time of northing. The observations were then continued at regular intervals, with two slight intermissions on account of intervening rocks, until about 2.45 a.m. (local apparent time), when the Sun had risen so high as to deprive them of their special interest. The point at which they were first commenced may be fixed with tolerable precision as 71° 2′ N. lat. and 1h. 45m. (26° 15′) E. long. The end of the series may be fixed, with somewhat less certainty, for 70° 55′ N. lat., 1h. 37m. (24° 15′) E. long. I subjoin a few considerations which seem to prove that the observations, comparatively speaking, are accurate and trustworthy.

(1) The Sun was very bright, and the sky almost cloudless throughout.

(2) The Index error of the instrument was marked before and after taking the observations: it seems to have shifted from about 50″, off the limb, to 40″, off the limb, i.e. through about 10″, while I was at work.

(3) The Chronometer was set by telegraphic signal, July 27th at Vardö, Aug. 10th and 21st at Christiania, Aug. 26th and 31st at Cambridge, and gave an uniform rate throughout, allowing for change of temperature.

(4) The ship's course from the time that the Sun would have been visible from behind the cliffs after leaving the harbour of Kielvig, on to the North Cape, and as far as Gjœsvör, is unquestionable: after that it is a little uncertain, especially after leaving a place named Havösund, but not seriously so.

(5) The Long. and Lat. throughout are certain. They depend indeed directly on a Norwegian survey of 1848: but this may be taken as proved correct, 1st from the nearness with which the situation of Vardö (another place not very distant), as given in it, corresponds with that obtained by repeated observations of my own; 2ndly from the near agreement between the position of the North Cape as laid down by the Norwegians, and that given by Mr Bayly, who was sent there in 1769 by the Royal Society. Mr Bayly was accompanied by Mr Dixon, who stationed himself at Hammerfest, about 40 miles to the south-west; and who is also known from having been sent to the Cape of Good Hope in 1761, and at a later date to Pennsylvania, for astronomical objects. Mr Bayly made the place of his own observatory 71° 0′ 4″·5 N., 26° 1½′ E., the chart making it, say, 71° 1′, 25° 55′ E.; and the North Cape 71° 10′ N., 25° 49′ E., the chart giving 71° 10′ N., 25° 46′ E.

(6) The dip, or amount to be subtracted from the altitude above the sea-horizon, owing to the height of the deck and the observer's eye, was carefully ascertained.

(7) Though there was for some time a thin oblong cloud, about the area of the Sun's disk, near the Sun, nothing in the way of haze or cloud ever obscured the limb observed.

The observed altitudes, times, &c., &c., are as follows: sufficient details are given to ascertain the true position of the Sun in every way in each case.

(1) *Before the Sun crossed the northern meridian.*

| | L. App. T. P.M. | Obs. Alt. | True Obs. Z. D. of ⊙'s centre. | Theor. Z. D. of ⊙'s centre. | Error. | Obs. Alt. too |
|---|---|---|---|---|---|---|
| (1) | 10h 6m 24s | 1° 44′ 40″ $\overline{\odot}$ | 88° 54′ 53″ | 88° 51′ 22″ | 3′ 31″ | low |
| (2) | „ 10 51 | 1 8 40 $\underline{\odot}$ | 89 3 2 | 89 0 37 | 2 25 | „ |
| (3) | „ 16 23 | 1 2 10 $\underline{\odot}$ | „ 10 20 | „ 10 33 | „ 13 | high |
| (4) | „ 24 42 | „ 48 20 $\underline{\odot}$ | „ 26 2 | „ 24 58 | 1 4 | low |
| (5) (6) | „ 33 46 | „ 31 15 $\underline{\odot}$ | „ 45 43 | „ 40 2 | 5 41 | „ |
| (7) | „ 42 7 | „ 21 10 $\underline{\odot}$ | „ 57 34 | „ 52 15 | 5 19 | „ |
| (8) | „ 49 43 | „ 37 50 $\overline{\odot}$ | 90 9 42 | 90° 2 29 | 7 13 | „ |
| (9) | „ 54 42 | „ 30 0 $\overline{\odot}$ | „ 18 48 | „ 9 0 | 9 48 | „ |
| (10) | 11 5 51 | „ 17 45 $\overline{\odot}$ | „ 33 12 | „ 22 2 | 11 10 | „ |
| (11) (12) | „ 14 41 | „ 9 45 $\overline{\odot}$ | „ 42 1 | „ 30 56 | 11 5 | „ |
| (13) (14) | „ 26 10 | „ 2 50 $\overline{\odot}$ | „ 51 6 | „ 39 54 | 11 12 | „ |
| (15) (16) | „ 33 43 | „ 1 35 $\overline{\odot}$ | „ 52 37 | „ 45 17 | 7 20 | „ |

(2) *After the Sun passed the northern meridian.*

| | L. App. T. A.M. | Obs. Alt. | True Obs. Z. D. of ⊙'s centre. | Theor. Z. D. of ⊙'s centre. | Error. | Obs. Alt. too |
|---|---|---|---|---|---|---|
| (17) | 12h 40m 41s | „ 5′ 50″ $\overline{\odot}$ | 90° 47′ 27″ | 90° 40′ 7″ | 7′ 20″ | low |
| (18) | 12 51 51 | „ 12 10 $\overline{\odot}$ | „ 39 50 | „ 30 14 | 9 36 | „ |
| (19) | 1 0 51 | „ 20 30 $\overline{\odot}$ | „ 29 57 | „ 20 32 | 9 25 | „ |
| (20) | 1 21 56 | „ 43 30 $\overline{\odot}$ | „ 3 10 | 89 52 10 | 11 0 | „ |
| (21) | 1 26 55 | „ 51 40 $\overline{\odot}$ | 89 53 50 | „ 45 22 | 8 28 | „ |
| (22) | 1 29 48 | „ 56 0 $\overline{\odot}$ | „ 48 54 | „ 40 55 | 7 59 | „ |
| (23) | 1 33 39 | 1 1 0 $\overline{\odot}$ | „ 43 15 | „ 35 14 | 8 1 | „ |
| (24) | 1 37 10 | 1 8 20 $\overline{\odot}$ | „ 35 1 | „ 28 45 | 6 16 | „ |
| (25) | 1 39 36 | „ 42 30 $\underline{\odot}$ | „ 32 43 | „ 24 28 | 8 15 | „ |
| (26) | 1 42 48 | „ 48 50 $\underline{\odot}$ | „ 25 28 | „ 18 43 | 6 45 | „ |
| (27) | 2 5 29 | 1 36 30 $\underline{\odot}$ | 88 32 2 | 88 28 30 | 3 32 | „ |
| (28) | 2 11 59 | 1 50 50 $\underline{\odot}$ | „ 16 34 | „ 12 28 | 4 6 | „ |
| (29) | 2 35 45 | 2 50 30 $\underline{\odot}$ | 87 12 41 | 87 9 56 | 2 45 | „ |
| (30) | 2 38 44 | 2 59 40 $\underline{\odot}$ | „ 3 3 | „ 1 50 | 1 13 | „ |
| (31) | 2 42 38 | 3 8 30 $\underline{\odot}$ | 86 51 45 | 86 50 26 | 1 19 | „ |
| (32) | 2 47 10 | 3 21 50 $\underline{\odot}$ | „ 38 10 | „ 37 8 | 1 2 | „ |

It may be added here that the barometer, an aneroid, stood throughout at 30·25. My thermometer, unfortunately, was packed up, and, I think, not accessible; but I am sure that the temperature was not far from 50 F. During the preceding six weeks I had repeatedly noted it between 11 P.M. and 1 A.M. at Vardö: this, and my constant habit of observing it at home enable me to say so much with confidence.

The Tables of refraction used in order to obtain the reduced or true position of the Sun's centre, corresponding to the observed altitude of his limb, are those of Ivory, which extend to a zenith distance of 90°. Bessel's Tables are not, strictly speaking, computed so far, but only to 85°; the supplement to them, for zenith distances between 85° and 89½°, is said by him, in the Preface to his *Tabulæ Regiomontanæ*, to depend considerably on observations of stars taken, not on the meridian, but when setting, on the western horizon, where it may be fairly said that their position is not certain, but constructive[1]. It is clearly however of little importance which Tables are used, as for 85° the two tables give the same result within a second: for 89° 11′ 40″ (for example) Th. 50° F., Bar. 30·25, the refraction on Ivory's Tables is 26′ 14″; on those of Bessel 26′ 44″ for 89° 28′ 45″, Ivory gives 28′ 51″, Bessel 29′ 31″. Whatever therefore may be the comparative merits of these two authorities, it is clear that they will not practically affect my own results.

The main feature to be noticed in these observations is that although the observed point of the Sun while more than a degree above the visible sea-horizon does not seem to have been more seriously displaced than might have been expected considering the uncertainty of the ship's position and the circumstances under which the sights were taken, still that below that altitude, and especially when the ship's latitude becomes almost absolutely certain on account of its proximity to so marked and known a point as the North Cape, the errors become very large indeed, and uniformly of one type, *i.e.* they invariably make the refraction less than it ought to be, the Sun's disk as observed being always lower, at the extreme point by as much as eleven minutes, than is required by the theories of the writers on this question. One simple consideration will prove that the phenomenon as observed could not have been in any way illusory: it will easily be found that in lat. 71° 10′, with the Sun's declination taken at 18° north, very nearly what it was on the evening in question, at intervals of 5, 10, 20, 40 minutes, before or after the Sun's passing the

[1] By this I mean that the Declination, and from that the refraction of any celestial object is, as a rule, determined from its altitude on the meridian. But in other parts of the sky, its proper altitude must be ascertained by geometry; *i.e.* is constructive.

northern meridian, its change in altitude is 15″, 1′ 0″, 4′ 1″, 16′ 2″, respectively. Now in the latitude of the North Cape, say 71° 11′, with the Sun's declination 17° 57′ 50″, as it must have been nearly, the depression of its centre below the horizon must have been about 51′ 10″. Allowing 15′ 48″ for its semidiameter, and 4′ 5″ for the dip, this is reduced to 31′ 17″. According to Ivory, the normal refraction for Z.D. 90° (I do not claim, as I ought, for 90° 4′ 5″) is 33′ 24″, or augmented for the barometer at 30·25, about 34′ 50″; in which case about $3\frac{1}{2}$ minutes of the Sun's upper limb ought to have been visible; now about 45 minutes before northing its altitude (upper limb) was only 9′ 50″, and, what is more, by 35 minutes before, had dropped to 2′ 50″, lingering I own at what, as far as I could judge, was about an altitude of 1′ for some time, though from the reflected rays the true horizon of the sea was extremely doubtful. It is almost impossible to give any explanation which will account for the disk of the Sun being so depressed, except by an actual change in the refraction of the atmosphere.

I will now mention something which leads me to think (for I cannot build a stronger argument on a single example) that the refraction for very small altitudes is extremely uncertain. On the previous evening, about 12 o'clock, we were passing a fine and well-defined headland named Nordkyn, about 30 miles E. by S. of the North Cape, and it occurred to me to take the Sun's altitude, which for the upper limb proved to be 16′ 40″, the time, 12h. 24m. 55s., local apparent time, being accurately noted. I did not take more than one sight; but on working it out twice very carefully with the help of the chart, I found the theoretical and observed altitudes of the Sun to coincide within a very few seconds. It is certain that the influence of the Gulf Stream both on the water of the sea and on the atmosphere diminishes very rapidly to the east of the North Cape; whether so great a change in the refraction can be attributed to a change in the state of the atmosphere, it is not for me to decide; but I can say this, that no pains were spared by myself to take my observations carefully.

Yet, on the other hand, with a constant temperature and barometer, the refraction seems almost certain. In the Monthly Notices of the Royal Astronomical Society for the year 1863 (vol. XXIII. p. 58) and for 1864 (vol. XXIV. p. 26) there are given two series of observations taken by Sir A. Lang on the disk of the Sun when rising above the sea-horizon, at St Croix in the West Indies, at an elevation of 440 feet above the sea level. The temperature throughout was extremely uniform, only varying between 73° F. and 79°·5, with a mean of 77°·5 in the first set of 17 observations (which extended over many years, from 1831 to 1848), and between 76° and 79°·3, with a mean of 76°·94; in the second set of 18,

taken from 1860 to 1863, the barometer also varied but slightly, from 29·53 to 29·70 in the first set, with a mean 29·619; and 29·36 to 29·71, with a mean of 29·63, in the second set. The computed refraction for the first set varied between 31′ 58″ and 33′ 1″, with a mean of 32′ 28″; for the second set between 32′ 10″ and 32′ 49″, with a mean of 32′ 14″. The uniformity of these results is remarkable; it need only be observed that for a zenith distance of 90° 21′, with the barometer at 30 inches and the thermometer at 50 F., the normal refraction would be about, according to Ivory's theory, 39′ 20″, which by the low barometric pressure and high temperature in the case in question, would be according to the same writer reduced by about 4′, to about 35′ 20″, or about 3′ more than actually observed.

These observations taken by Sir A. Lang seem to be the most striking examples of horizontal refraction that I have been able to find in publications on Astronomy. But in Bessel's *Fundamenta Astronomiæ*, p. 53, there will be found seven observations of the star Lyra, taken by Bradley at the Royal Observatory at Greenwich in the years 1750—2, at a zenith distance of about 89° 30′. According to Bessel, the errors between the observed and theoretical positions of the star was in one case nothing, and in only one more than 1′, when it was 1′ 16″. These last observations may perhaps be thought by some to militate strongly against the accuracy of my own observations, but, without entering on any other points, it seems to me not unlikely that at very low altitudes the refraction sustained by the huge disk of the Sun would not be the same as that of a pointed ray (if I may be allowed the phrase) such as that emanating from a fixed star: or, to speak more precisely, may it not be possible that the wave undulations originating in so large a volume of light as that of the Sun, when transmitted through a long series of strata of atmosphere perhaps unequally warmed, may be affected by refraction very differently from those originating in a star, even of the first magnitude?

Brinkley and Plana have also given examples of stars observed at low altitudes: the former at an altitude of 2° 18′, the latter of 1° 36′; in which cases the observed and theoretical positions of the star are said to have harmonized within a very few seconds; but it will be seen that in neither of these cases is the altitude nearly so small as in my own observations[1].

I wish it to be fully understood that I do not wish to throw any doubt on the accuracy of the laws of refraction hitherto adopted by astronomers and mathematicians down to an altitude of about 3°, or even lower. During the months of June and July

[1] For Brinkley's observations, see *Transactions of the R. Irish Academy*, 1820; vol. XIII. p. 172. Plana's I have not been able to find, but they are referred to in the Introduction to Shortrede's *Logarithmic Tables*, Introd. p. 12.

I was able to get five sets of observations of the Sun on the northern meridian with a six-inch theodolite graduated to 20″, used as an altazimuth, and also one good set with my prism-circle on the sea-horizon, at Vardö or Wardhuis, in lat. 70° 22′ 30″. The last set of 12 sights gave the Sun's altitude a little (1′ 6″) too high, with tolerable uniformity: the Sun's altitude (centre) being as nearly as possible 4°. With the theodolite, on the first night, with the Sun's centre about 3° 45′, its place came out again about 1′ 0″ too high; on the next, with the Sun's upper limb (the only one observed) about 3°, the error was 37″ too high. The third set (Sun's centre about 2° 20′) gave, on account of clouds, a good mean result, but with great discrepancies. The next evening (Sun's centre 2° 15′) gave for 12 observations a mean result of 37″ too low. The last set (Sun's centre 2° 0′) gave an error of 27″ too high on the upper limb, and 25″ too low on the lower limb. The observations with the theodolite, it may be added, gave uniformly the position of the Sun lower on the lower limb than on the upper one, and as this could not well be due to the instrument, the position of the Sun being marked by taking its disk when in contact with the point where the cross wires intersect on the horizontal wire, it seems possible to suppose that the refraction on either limb of the Sun was not the refraction actually due to that required by the altitude of that limb, but rather that due to some intermediate point nearer the Sun's centre, which will account more or less for the observed error. My reason for referring to these observations is because, depending as they do on the adjustment of the instrument by a spirit-level, and not on the sea-horizon, one element of uncertainty is eliminated; and secondly because they prove, if proof is needed, that it is only within the narrow limit which I have defined already as a degree and a half or two degrees, that the accepted laws of refraction seem to become seriously uncertain in their application.

The problem of horizontal refraction is one which is far best subjected to practical observation in very high latitudes, on account of the nearly horizontal motion of the objects observed when near the meridian, as I have already pointed out, pp. 355, 6. Had I been able to find a suitable station to the West instead of to the East of the North Cape, I have reason to think that the sky at night would have been clearer than I found it where I stationed myself, and that I might have been rewarded with a better series of observations with my theodolite of the Sun's motion in altitude and azimuth simultaneously, which was the main object which I had in view when I went, and which I regret to say was to a great extent a failure, owing to the sky being almost always obscured near the horizon when the Sun was at its lowest.

(3) C. TAYLOR, M.A. *On a section of Newton's Principia in relation to Modern Geometry.*

The following notes on the fifth section of the first book of Newton's *Principia* comprise *inter alia* some particulars (§ 3) which I have not seen stated before.

1. The section commences with what may be called the Tetragram of Pappus, *i.e.* the theorem (handed down by him without solution) that the product of the distances of any point on a conic from two opposite sides of a fixed inscribed quadrilateral varies as the product of its distances from the other two sides. Descartes proved this only by his new method of co-ordinates: Newton here proves it by the most elementary geometry. It is well known that *the anharmonic property of four points on a conic* is merely another way of stating the Tetragram of Pappus.

2. From this theorem Newton deduces his own organic description of a conic by means of two rotating angles of given magnitudes. It is easy to see that we have here again, under another form, the same anharmonic property.

3. *The anharmonic property of tangents to a conic.*

Newton shews in a corollary to Lemma 25 that if the sides $IM$, $ML$, $LK$, $KI$ of a fixed parallelogram circumscribed to a given conic be met by any fifth tangent in points $EFHQ$, then

$$KQ \, . \, ME = \text{a constant} = KH \, . \, MF.$$

*a.* Hence ($K$ and $M$ being fixed points on the tangents from a fixed point $I$) *any two fixed tangents to a conic are divided homographically by a variable tangent.*

*b.* It is likewise evident that the intercepts $IE$ and $IQ$ are connected by a relation of the form,

$$a \, . \, IE \, . \, IQ + b \, . \, IE + c \, . \, IQ + d = 0,$$

which is a "tangential equation" to the conic referred to any given pair of tangents.

*c.* From the constancy of the rectangle $KQ \, . \, ME$, and in like manner of $QI \, . \, LH$, it follows that *the ratio of the ratios* $\frac{FE}{FH}$ *and* $\frac{QE}{QH}$ *is constant.*

This anharmonic property of four fixed tangents, being projective, is true for any form of the quadrilateral determined by the fixed tangents, and not for a parallelogram only.

*d.* It follows also that *the ratio of the ratios* $\left(\frac{QK.FM}{QI.FL}\right)$ *in which the variable tangent divides either pair of opposite sides of IMLK is constant.* And this ratio of ratios will still be constant (although not of the same magnitude as before) if the figure be projected from any vertex upon any plane, so that *IMLK* becomes a quadrilateral of any other form. The theorem in question may also be stated as follows:

*The ratios of the products of the distances of any tangent to a conic from the three pairs of opposite summits of a given circumscribed quadrilateral are constant.*

4. In the same Lemma 25, Cor. 3, it is proved that *the centres of all conics inscribed in a given quadrilateral are collinear.* This important and suggestive theorem served as a starting point for the investigation of the properties of systems of conics subject to four conditions. Notice the use made of it by Brianchon and Poncelet in Gergonne's *Annales*, XI. 219.

5. It is well known that a general method of transforming curves is briefly but adequately laid down in Lemma 22.

Thus the section under consideration contains the fundamental propositions and methods on which so much of the modern geometry is founded. The subject deserves to be treated at greater length; but suffice it for the present to call attention to Newton's proof of the important general property, § 3, of tangents to a conic, which Chasles appears to have overlooked in his note *Sur la propriété anharmonique des tangentes d'une conique* (*Aperçu Historique*, Note XVI., pp. 341—344, edit. 2).

I may remark in conclusion, that Lambert's solution of the problem[1]—sometimes called Sir Christopher Wren's problem—*to draw a straight line whose segments by four given straight lines shall be in given ratios,* contains the anharmonic property in question. Lambert shews (*Insigniores orbitæ cometarum proprietates*, Sect. I. Lemma 18) that the envelope of the line so divided is the parabola which touches the four given lines. This evidently includes the anharmonic property of four tangents to a parabola, which may be at once extended to the general conic by projection.

[1] He refers to Newton for another solution, and Newton (sect. v. lemma 27, cor.) refers to Wren and Wallis.

(4) A. G. GREENHILL, M.A., *Integrals expressed by Inverse Elliptic Functions.*

If $\int_0^x \frac{dx}{\sqrt{(1-x^2)}} = u$, then $x = \sin u$, and $u$, the inverse function, is denoted by $\sin^{-1} x$ by English writers, and by arc $\sin x$ by continental writers.

If $\int_1^x \frac{dx}{\sqrt{(x^2-1)}} = u$, then $x = \frac{1}{2}(e^u + e^{-u}) = \cosh u$, and $u$, the inverse function, is denoted by $\log\{x + \sqrt{(x^2-1)}\}$, a notation universally employed.

In ordinary treatises on the Integral Calculus it is shewn how the integral of any rational expression involving $\sqrt{R}$, where $R$ is of the first or second degree, leads to either of the above forms and to algebraical expressions.

When $R$ is of the third or fourth degree, elliptic integrals are introduced; and from the definition of the function,

if
$$\int_0^x \frac{dx}{\sqrt{(1-x^2 \,.\, 1-k^2x^2)}} = u,$$
then
$$x = \operatorname{sn}(u, k).$$

To express $u$, the inverse function, in terms of $x$, we may either use a notation employed by Gudermann, analogous to the continental arc sin, and put

$$u = \arg \operatorname{sn}(x, k),$$

or we may use the English notation and put

$$u = \operatorname{sn}^{-1}(x, k).$$

Then, from the definitions of the functions, $(x^2 < 1)$,

$$(1) \quad \int_0^x \frac{dx}{\sqrt{(1-x^2 \,.\, 1-k^2x^2)}} = \operatorname{sn}^{-1}(x, k) \text{ or } \arg \operatorname{sn}(x, k),$$

$$(2) \quad \int_x^1 \frac{dx}{\sqrt{(1-x^2 \,.\, k'^2+k^2x^2)}} = \operatorname{cn}^{-1}(x, k) \text{ or } \arg \operatorname{cn}(x, k),$$

$$(3) \quad \int_x^1 \frac{dx}{\sqrt{(1-x^2 \,.\, x^2-k'^2)}} = \operatorname{dn}^{-1}(x, k) \text{ or } \arg \operatorname{dn}(x, k).$$

The results of the substitutions to reduce elliptic integrals to their normal form are now clearly exhibited when the elliptic integrals are expressed by inverse elliptic functions, the form of the result indicating the substitution to be employed.

Thus, $a > b$,

(4) $$\int_0^x \frac{dx}{\sqrt{(a^2 - x^2 \,.\, b^2 - x^2)}} = \frac{1}{a} \arg \operatorname{sn} \left(\frac{x}{b},\ \frac{b}{a}\right), \text{ if } x < b;$$

indicating that we must put

$$\sin\phi = \frac{x}{b};$$

and then the integral becomes

$$\frac{1}{a}\int \frac{d\phi}{\sqrt{\left(1 - \frac{b^2}{a^2}\sin^2\phi\right)}}.$$

(5) $$\int_x^\infty \frac{dx}{\sqrt{(x^2 - a^2 \,.\, x^2 - b^2)}} = \frac{1}{a} \arg \operatorname{sn} \left(\frac{a}{x},\ \frac{b}{a}\right), \text{ if } x > a;$$

(6) $$\int_x^b \frac{dx}{\sqrt{(a^2 + x^2 \,.\, b^2 - x^2)}} = \frac{1}{\sqrt{(a^2 + b^2)}} \arg \operatorname{cn} \left\{\frac{x}{b},\ \frac{b}{\sqrt{(a^2 + b^2)}}\right\};$$

(7) $$\int_b^x \frac{dx}{\sqrt{(a^2 + x^2 \,.\, x^2 - b^2)}} = \frac{1}{\sqrt{(a^2 + b^2)}} \arg \operatorname{cn} \left\{\frac{b}{x},\ \frac{a}{\sqrt{(a^2 + b^2)}}\right\};$$

(8) $$\int_x^a \frac{dx}{\sqrt{(a^2 - x^2 \,.\, x^2 - b^2)}} = \frac{1}{a} \arg \operatorname{dn} \left\{\frac{x}{a},\ \sqrt{\left(1 - \frac{b^2}{a^2}\right)}\right\};$$

(9) $$\int_0 \frac{dx}{\sqrt{(x^2 + a^2 \,.\, x^2 + b^2)}} = \frac{1}{a} \arg \operatorname{tn} \left\{\frac{x}{b},\ \sqrt{\left(1 - \frac{b^2}{a^2}\right)}\right\}.$$

As examples,

$$\int_x^1 \frac{dx}{\sqrt{(1-x^4)}} = \frac{1}{\sqrt{2}} \arg \operatorname{cn} \left(x,\ \frac{1}{\sqrt{2}}\right),$$

$$\int_1^x \frac{dx}{\sqrt{(x^4-1)}} = \frac{1}{\sqrt{2}} \arg \operatorname{cn} \left(\frac{1}{x},\ \frac{1}{\sqrt{2}}\right).$$

Again, $a > b > c > d$,

(10) $$\int \frac{dx}{\sqrt{(x - a \,.\, x - b \,.\, x - c \,.\, x - d)}},$$

$$= \frac{2}{\sqrt{(a - c \,.\, b - d)}} \arg \operatorname{sn} \sqrt{\frac{x - a \,.\, b - d}{x - b \,.\, a - d}},$$

indicating that we must put

$$\sin\phi = \sqrt{\frac{x - a \,.\, b - d}{x - b \,.\, a - d}},$$

and then the integral becomes

$$\frac{2}{\sqrt{(a-c\,.\,b-d)}}\int\frac{d\phi}{\sqrt{(1-k^2\sin^2\phi)}};$$

$$=\frac{2}{\sqrt{(a-c\,.\,b-d)}}\arg\operatorname{cn}\sqrt{\frac{x-d\,.\,a-b}{x-b\,.\,a-d}}$$

$$=\frac{2}{\sqrt{(a-c\,.\,b-d)}}\arg\operatorname{dn}\sqrt{\frac{x-c\,.\,a-b}{x-b\,.\,a-c}}.$$

$$=\frac{1}{\sqrt{(a-c.b-d)}}\arg\operatorname{sn}\frac{2\sqrt{(x-a\,.\,x-b\,.\,x-c\,.\,x-d\,.\,a-c\,.\,b-d)}}{(a+b-c-d)x^2-2(ab-cd)x+ab(c+d)-cd(a+b)},$$

if $$\infty>x>a,\text{ or } d>x>-\infty,$$

and $$k^2=\frac{b-c\,.\,a-d}{a-c\,.\,b-d}.$$

(11) $$\int\frac{dx}{\sqrt{(x-a\,.\,x-b\,.\,x-c\,.\,x-d)}}$$

$$=\frac{2}{\sqrt{(a-c\,.\,b-d)}}\arg\operatorname{sn}\sqrt{\frac{x-c\,.\,b-d}{x-d\,.\,b-c}}$$

$$=\frac{2}{\sqrt{(a-c\,.\,b-d)}}\arg\operatorname{cn}\sqrt{\frac{b-x\,.\,c-d}{x-d\,.\,b-c}}$$

$$=\frac{2}{\sqrt{(a-c\,.\,b-d)}}\arg\operatorname{dn}\sqrt{\frac{a-x\,.\,c-d}{x-d\,.\,a-c}}$$

$$=\frac{1}{\sqrt{(a-c.b-d)}}\arg\operatorname{sn}\frac{2\sqrt{(x-a\,.\,x-b\,.\,x-c\,.\,x-d\,.\,a-c\,.\,b-d)}}{-(a+b-c-d)x^2+2(ab-cd)x-ab(c+d)+cd(a+b)},$$

if $$a>x>b,$$

and $$k^2=\frac{b-c\,.\,a-d}{a-c\,.\,b-d}.$$

(12) $$\int\frac{dx}{\sqrt{-(x-a\,.\,x-b\,.\,x-c\,.\,x-d)}}$$

$$=\frac{2}{\sqrt{(a-c\,.\,b-d)}}\arg\operatorname{sn}\left(\sqrt{\frac{x-d\,.\,a-c}{a-x\,.\,c-d}},\ \sqrt{\frac{a-b\,.\,c-d}{a-c\,.\,b-d}}\right),$$

if $$c>x>d.$$

(13) $$\int\frac{dx}{\sqrt{-(x-a\,.\,x-b\,.\,x-c\,.\,x-d)}}$$

$$=\frac{2}{\sqrt{(a-c\,.\,b-d)}}\arg\operatorname{sn}\left(\sqrt{\frac{x-b\,.\,a-c}{x-c\,.\,a-b}},\ \sqrt{\frac{a-b\,.\,c-d}{a-c\,.\,b-d}}\right),$$

if $$a>x>b.$$

Again, $$a > b > c,$$

(14) $$\int_a^x \frac{dx}{\sqrt{(x-a \,.\, x-b \,.\, x-c)}} = \frac{2}{\sqrt{(a-c)}} \arg \operatorname{cn}\left(\sqrt{\frac{a-b}{x-b}}, \sqrt{\frac{b-c}{a-c}}\right),$$

if $$\infty > x > a.$$

(15) $$\int_c^x \frac{dx}{\sqrt{(x-a \,.\, x-b \,.\, x-c)}} = \frac{2}{\sqrt{(a-c)}} \arg \operatorname{sn}\left(\sqrt{\frac{x-c}{b-c}}, \sqrt{\frac{b-c}{a-c}}\right),$$

$$b > x > c.$$

(16) $$\int_{-\infty}^x \frac{dx}{\sqrt{-(x-a \,.\, x-b \,.\, x-c)}}$$

$$= \frac{2}{\sqrt{(a-c)}} \arg \operatorname{sn}\left(\sqrt{\frac{a-c}{a-x}}, \sqrt{\frac{a-b}{a-c}}\right),$$

$$c < x > -\infty.$$

(17) $$\int_b^x \frac{dx}{\sqrt{-(x-a \,.\, x-b \,.\, x-c)}}$$

$$= \frac{2}{\sqrt{(a-c)}} \arg \operatorname{dn}\left(\sqrt{\frac{b-c}{x-c}}, \sqrt{\frac{a-b}{a-c}}\right),$$

$$a > x > b.$$

For example

$$\int_\lambda^\infty \frac{d\lambda}{\sqrt{(a^2+\lambda \,.\, b^2+\lambda \,.\, c^2+\lambda)}}$$

$$= \frac{2}{\sqrt{(a^2-c^2)}} \arg \operatorname{cn}\left(\sqrt{\frac{c^2+\lambda}{a^2+\lambda}}, \sqrt{\frac{a^2-b^2}{a^2-c^2}}\right),$$

an integral occurring in the theories of Attraction, Electricity, Magnetism, &c.

(18) $$\int \frac{dx}{\sqrt{[x-a \,.\, x-b \,.\, \{(x-m)^2+n^2\}]}}$$

$$= \frac{1}{\sqrt{\alpha\beta}} \arg \operatorname{cn}\left\{\frac{\alpha(x-b)-\beta(x-a)}{\alpha(x-b)+\beta(x-a)}, k\right\},$$

where $$\alpha^2 = (m-a)^2 + n^2,$$

$$\beta^2 = (m-b)^2 + n^2,$$

$$k^2 = \tfrac{1}{2}\left\{1 + \frac{\alpha^2+\beta^2-(a-b)^2}{2\alpha\beta}\right\}.$$

(19) $$\int_b^x \frac{dx}{\sqrt{[-(x-a)(x-b)\{(x-m)^2+n^2\}]}}$$

$$= \frac{1}{\sqrt{(\alpha\beta)}} \arg \operatorname{cn} \left\{\frac{\beta(x-a)+\alpha(x-b)}{\beta(x-a)-\alpha(x-b)},\ k\right\},$$

where $$k^2 = \tfrac{1}{2}\left\{1 - \frac{\alpha^2+\beta^2-(a-b)^2}{2\alpha\beta}\right\}.$$

(20) $$\int_a^x \frac{dx}{\sqrt{[(x-a)\{(x-m)^2+n^2\}]}}$$

$$= \frac{1}{\sqrt{\alpha}} \arg \operatorname{cn} \left\{\frac{\alpha-(x-a)}{\alpha+(x-a)},\ k\right\},$$

where $$(a-m)^2+n^2 = \alpha^2,$$

$$k^2 = \tfrac{1}{2}\left(1 + \frac{m-a}{\alpha}\right).$$

(21) $$\int \frac{dx}{\sqrt{[-(x-a)\{(x-m)^2+n^2\}]}}$$

$$= \frac{1}{\sqrt{a}} \arg \operatorname{cn} \left(\frac{x-a+\alpha}{x-a-\alpha},\ k\right),$$

where $$k^2 = \tfrac{1}{2}\left(1 - \frac{m-a}{\alpha}\right).$$

(22) $$\int \frac{dx}{\sqrt{\{(x-m)^2+n^2\}\{(x-m')^2+n'^2\}}}$$

is reduced by putting

$$x - m = n\tan(\phi-\omega),$$

and then =

$$\int \frac{d\phi}{\sqrt{[\{(m-m')^2+n^2+n'^2\}+\frac{1}{2}\{(m-m')^2-n^2+n'^2\}\cos 2(\phi-\omega)-n(m-m')\sin 2(\phi-\omega)]}}.$$

The coefficient of $\sin 2\phi$ is made to vanish by putting

$$\tan 2\omega = \frac{2n(m-m')}{(m-m')^2-n^2+n'^2},$$

and then, if

$$r^2 = (m-m')^2+(n+n')^2,$$

$$s^2 = (m-m')^2+(n-n')^2;$$

the integral

$$= 2\int \frac{d\phi}{\sqrt{(r^2+2rs\cos 2\phi+s^2)}}$$

$$= \frac{2}{r+s}\int \frac{d\phi}{\sqrt{(1-k^2\sin^2\phi)}}, \quad k^2 = \frac{4rs}{(r+s)^2}$$

$$= \frac{2}{r+s}\arg\operatorname{tn}\left\{\frac{x-m+n\tan\omega}{n-(x-m)\tan\omega}, \quad \frac{2\sqrt{(rs)}}{r+s}\right\}.$$

For instance, as particular cases of (22),

$$\int_0^x \frac{dx}{\sqrt{(1+x^4)}} = \tfrac{1}{2}\arg\operatorname{cn}\left(\frac{1-x^2}{1+x^2}, \quad \frac{1}{\sqrt{2}}\right),$$

$$\int_0^x \frac{dx}{\sqrt{(x^4+3x^2+3)}} = \frac{1}{2\sqrt[4]{3}}\arg\operatorname{cn}\left(\frac{\sqrt{3}-x^2}{\sqrt{3}+x^2}, \quad \sin 15^\circ\right).$$

Thus
$$\int \frac{dx}{\sqrt{(a+bx+cx^2+dx^3+ex^4)}},$$

which can be reduced to one of the forms (10) to (22), can be expressed by inverse elliptic functions.

By the inverse notation the results of the integration of all integrals, a number of which are considered in Legendre's *Fonctions Elliptiques,* which by substitution can be reduced to elliptic integrals, can be written down; thus as examples

$$\int_x^1 \frac{dx}{\sqrt{(1-x^6)}} = \frac{1}{2\sqrt[4]{3}}\arg\operatorname{cn}\left\{\frac{(\sqrt{3}+1)\,x^2-1}{(\sqrt{3}-1)\,x^2+1}, \quad \sin 15^\circ\right\},$$

$$\int_x^{2^{-\frac{1}{6}}} (1-x^6)^{-\frac{5}{6}}\,dx\;(2^{-\frac{1}{6}} > x > 0) = \int_{2^{-\frac{1}{6}}}^x (1-x^6)^{-\frac{5}{6}}\,dx\;(1 > x > 2^{-\frac{1}{6}})$$

$$= \frac{\sqrt[3]{4}}{2\sqrt[4]{3}}\arg\operatorname{cn}\left\{\frac{(\sqrt{3}+1)\sqrt[3]{4}x^2(1-x^6)^{\frac{1}{3}}-1}{(\sqrt{3}-1)\sqrt[3]{4}x^2(1-x^6)^{\frac{1}{3}}+1}, \quad \sin 15^\circ\right\}$$

(Bertrand, *Calcul Intégral,* p. 686).

$$\int_0^x (1+x^2)^{-\frac{3}{4}}\,dx = \sqrt{2}\arg\operatorname{cn}\left\{(1+x^2)^{-\frac{1}{4}}, \quad \frac{1}{\sqrt{2}}\right\},$$

$$\int_0^x (1-x^2)^{-\frac{3}{4}}\,dx = \sqrt{2}\arg\operatorname{cn}\left\{(1-x^2)^{-\frac{1}{4}}, \quad \frac{1}{\sqrt{2}}\right\},$$

$$\int_1^x (x^2-1)^{-\frac{3}{4}}\,dx = \quad \arg\operatorname{cn}\left\{\frac{1-\sqrt{x^2-1}}{1+\sqrt{x^2-1}}, \quad \frac{1}{\sqrt{2}}\right\},$$

$$\int (x-a\,.\,x-b)^{-\frac{3}{4}}\,dx = \sqrt{\left(\frac{2}{a-b}\right)}\ \text{arg cn}\left(\frac{a-b-2\sqrt{x-a\,.\,x-b}}{a-b+2\sqrt{x-a\,.\,x-b}},\ \frac{1}{\sqrt{2}}\right),$$

$$\int (a-x\,.\,x-b)^{-\frac{3}{4}}\,dx = \frac{2}{\sqrt{(a-b)}}\ \text{arg cn}\left\{\frac{(a-x\,.\,x-b)^{\frac{1}{4}}}{\sqrt{\{\frac{1}{2}(a-b)\}}},\ \frac{1}{\sqrt{2}}\right\},$$

$$\int \{(x-m)^2+n^2\}^{-\frac{3}{4}}\,dx = \frac{2}{\sqrt{n}}\ \text{arg cn}\left[\frac{\sqrt{n}}{\{(x-m)^2+n^2\}^{\frac{1}{4}}},\ \frac{1}{\sqrt{2}}\right].$$

$$\int_x^{2^{-\frac{1}{4}}} (1-x^4)^{-\frac{3}{4}}\,dx\ (2^{-\frac{1}{4}} > x > 0)$$

$$= \int_{2^{-\frac{1}{4}}}^{x} (1-x^4)^{-\frac{3}{4}}\,dx\ (1 > x > 2^{-\frac{1}{4}})$$

$$= \frac{\sqrt[3]{4}}{4\sqrt{2}}\ \text{arg cn}\left\{(\sqrt{2x}\,\sqrt[4]{(1-x^4)},\ \frac{1}{\sqrt{2}}\right\}.$$

$$(23)\qquad \int \frac{dx}{(x-a\,.\,x-b\,.\,x-c)^{\frac{2}{3}}}$$

$$= \frac{x^{\frac{3}{4}}}{(4\,.\,a-b\,.\,a-c\,.\,b-c)^{\frac{1}{3}}}$$

$$\text{arg cn}\left\{\frac{b-c\,.\,x-a)^{\frac{2}{3}}(\sqrt{3}-1)-(4\,.\,x-b\,.\,x-c\,.\,a-b\,.\,a-c)^{\frac{1}{3}}}{(b-c\,.\,x-a)^{\frac{2}{3}}(\sqrt{3}+1)+(4\,.\,x-b\,.\,x-c\,.\,a-b\,.\,a-c)^{\frac{1}{3}}},\ \sin 75^{\circ}\right\}.$$

$$(24)\qquad \frac{dx}{[(x-a)\,\{(x-m)^2+n^2\}]^{\frac{2}{3}}}$$

$$= \frac{3^{\frac{3}{4}}}{2n^{\frac{1}{3}}\,\{(a-m)^2+n^2\}^{\frac{1}{3}}}$$

$$\text{arg cn}\left[\frac{(n\,.\,x-a)^{\frac{2}{3}}(\sqrt{3}+1)-\{(x-m)^2+n^2\,.\,(a-m)^2+n^2\}^{\frac{1}{3}}}{(n\,.\,x-a)^{\frac{2}{3}}(\sqrt{3}-1)+\{(x-m)^2+n^2\,.\,(a-m)^2+n^2\}^{\frac{1}{3}}},\ \sin 15^{\circ}\right]$$

an integral occurring in the motion of a body moving under gravity in a medium in which the resistance varies as the cube of the velocity. The last two integrals (23) and (24) were considered by Allégret (*Comptes Rendus*, t. 66, p. 1144).

If $X = 1-x\,.\,1+\kappa x\,.\,1+\lambda x\,.\,1-\kappa\lambda x$ (Cayley, *Elliptic Functions*, Chap. XVI.),

$$\int_0^x \frac{dx}{\sqrt{xX}} = \frac{1}{\sqrt{(1+\kappa\,.\,1+\lambda)}}\left\{\arg \operatorname{sn}\left(\sqrt{\frac{1+\kappa\,.\,1+\lambda\,.\,x}{1+\kappa x\,.\,1+\lambda x}},\,b\right)\right.$$
$$\left.+\arg \operatorname{sn}\left(\sqrt{\frac{1+\kappa\,.\,1+\lambda\,.\,x}{1+\kappa x\,.\,1+\lambda x}},\,c\right)\right\},$$

$$\int_0^x \sqrt{\frac{x}{X}}\,dx = \frac{1}{\sqrt{(\kappa\lambda\,.\,1+\kappa\,.\,1+\lambda)}}\left\{\arg \operatorname{sn}\left(\sqrt{\frac{1+\kappa\,.\,1+\lambda\,.\,x}{1+\kappa x\,.\,1+\lambda x}},\,b\right)\right.$$
$$\left.-\arg \operatorname{sn}\left(\sqrt{\frac{1+\kappa\,.\,1+\lambda\,.\,x}{1+\kappa x\,.\,1+\lambda x}},\,c\right)\right\},$$

where

$$b = \frac{\sqrt{\kappa}+\sqrt{\lambda}}{\sqrt{(1+\kappa\,.\,1+\lambda)}},\qquad c = \frac{\sqrt{\kappa}-\sqrt{\lambda}}{\sqrt{(1+\kappa\,.\,1+\lambda)}}.$$

More generally (M. Roberts, *Tract on the Addition of Elliptic and Hyper-elliptic Integrals*, p. 53) putting $x = t^2$,

$$\int_0^t \frac{dt}{\sqrt{(1-pt^2+qt^4-pt^6+t^8)}}$$
$$= \tfrac{1}{2}\int_u^\infty \frac{du}{\sqrt{\{u^4-(p+4)\,u^2+2+2p+q\}}}$$
$$+\tfrac{1}{2}\int_v^\infty \frac{dv}{\sqrt{\{v^4-(p-4)\,v^2+2-2p+q\}}},$$

where

$$t+t^{-1}=u$$
$$t-t^{-1}=v$$
$$=\tfrac{1}{2}(U+V),\ \text{suppose};$$

and

$$\int_0^t \frac{t^2\,dt}{\sqrt{(1-pt^2+qt^4-pt^6+t^8)}}$$
$$=\tfrac{1}{2}(V-U).$$

Also

$$V = \frac{2}{\sqrt{\{4+p+\sqrt{(8-4q+p^2)}\}}}$$
$$\arg \operatorname{sn}\left\{\frac{\sqrt{\{4+p+\sqrt{(8-4q+p^2)}\}}\,t}{1+t^2},\ \sqrt{\frac{4+p-\sqrt{(8-4q+p^2)}}{4+p+\sqrt{(8-4q+p^2)}}}\right\},$$

$$U = \frac{2}{\sqrt{\{4-p+\sqrt{(8-4q+p^2)}\}}}$$
$$\arg \operatorname{sn}\left\{\frac{\sqrt{\{4-p+\sqrt{(8-4q+p^2)}\}}\,t}{1+t^2},\ \sqrt{\left\{\frac{4-p-\sqrt{(8-4q+p^2)}}{4-p+\sqrt{(8-4q+p^2)}}\right\}}\right\};$$

and generally (p. 82) the integral $\int \frac{\alpha+\beta x}{\sqrt{X}}\,dx$, where $X$ denotes a sextic function, whose skew-invariant vanishes, can be similarly expressed.

For if the skew-invariant vanishes, the roots of the sextic form an involution, and by a linear substitution we can make the sextic a reciprocal expression.

We have

$$\frac{d}{dx}\arg\operatorname{sn}\left(x\sqrt{\frac{1-x^2}{1+x^2}},\ \tan\tfrac{1}{8}\pi\right)$$

$$=\frac{1-(\sqrt{2}+1)\,x^2}{\sqrt{(1-x^8)}}\quad \text{if } x^2<\sqrt{2}-1<\tan\tfrac{1}{8}\pi;$$

$$=\frac{(\sqrt{2}+1)\,x^2-1}{\sqrt{(1-x^8)}}\quad \text{if } x^2>\sqrt{2}-1>\tan\tfrac{1}{8}\pi;$$

and

$$\frac{d}{dx}\arg\operatorname{sn}\left(\frac{x\sqrt{\dfrac{1-x^2}{1+x^2}}}{\tan\frac{1}{8}\pi},\ \tan\tfrac{1}{8}\pi\right)$$

$$=\frac{1+(\sqrt{2}-1)\,x^2}{\sqrt{1-x^8}}.$$

Therefore, $x<\sqrt{\tan\frac{1}{8}\pi}$,

$$\int_0^x\frac{dx}{\sqrt{1-x^8}}=\frac{\sqrt{2}-1}{2\sqrt{2}}\arg\operatorname{sn}x\sqrt{\frac{1-x^2}{1+x^2}}+\frac{\sqrt{2}+1}{2\sqrt{2}}\arg\operatorname{sn}\frac{x\sqrt{\dfrac{1-x^2}{1+x^2}}}{\tan\frac{1}{8}\pi},$$

$$\int_0^x\frac{x^2dx}{\sqrt{(1-x^8)}}=-\frac{1}{2\sqrt{2}}\arg\operatorname{sn}x\sqrt{\frac{1-x^2}{1+x^2}}+\frac{1}{2\sqrt{2}}\arg\operatorname{sn}\frac{x\sqrt{\dfrac{1-x^2}{1+x^2}}}{\tan\frac{1}{8}\pi};$$

and $x>\sqrt{\tan\frac{1}{8}\pi}$,

$$\int_x^1\frac{dx}{\sqrt{(1-x^8)}}=-\frac{\sqrt{2}-1}{2\sqrt{2}}\arg\operatorname{sn}x\sqrt{\frac{1-x^2}{1+x^2}}+\frac{\sqrt{2}+1}{2\sqrt{2}}\arg\operatorname{sn}\frac{x\sqrt{\dfrac{1-x^2}{1+x^2}}}{\tan\frac{1}{8}\pi}$$

$$\int_x^1\frac{x^2dx}{\sqrt{(1-x^8)}}=\frac{1}{2\sqrt{2}}\arg\operatorname{sn}x\sqrt{\frac{1-x^2}{1+x^2}}+\frac{1}{2\sqrt{2}}\arg\operatorname{sn}\frac{x\sqrt{\dfrac{1-x^2}{1+x^2}}}{\tan\frac{1}{8}\pi},$$

(Richelot, *Crelle*, t. 32, p. 213).

Again (Legendre, *Fonctions Elliptiques*, t. I. p. 178).

$$\int_0^\phi \frac{d\phi}{(1-k^2\sin^2\phi)^{\frac{1}{4}}} = 2\int_x^1 \frac{x^2dx}{\sqrt{(1-x^4 \,.\, x^4-k'^2)}} \text{ if } x^4 = 1-k^2\sin^2\phi,$$

$$= \frac{1}{\sqrt{(2+2k')}}\left[\arg\operatorname{cn}\left\{\frac{(1-k^2\sin^2\phi)^{\frac{1}{2}}-\sqrt{k'}}{(1-k^2\sin^2\phi)^{\frac{1}{4}}(1-\sqrt{k'})}, \frac{1-\sqrt{k'}}{\sqrt{(2+2k')}}\right\}\right.$$

$$\left. + \arg\operatorname{cn}\left\{\frac{(1-k^2\sin^2\phi)^{\frac{1}{2}}+\sqrt{k'}}{(1-k^2\sin^2\phi)^{\frac{3}{4}}(1+\sqrt{k'})}, \frac{1+\sqrt{k'}}{\sqrt{(2+2k')}}\right\}\right];$$

and $$\int_0^\phi \frac{d\phi}{(1-k^2\sin^2\phi)^{\frac{3}{4}}} = \int_x^1 \frac{2dx}{x\sqrt{(1-x^4 \,.\, x^4-k'^2)}}$$

$$= \frac{1}{\sqrt{(2k'+2k'^2)}}\left[-\arg\operatorname{cn}\left\{\frac{(1-k^2\sin^2\phi)^{\frac{1}{2}}-\sqrt{k'}}{(1-k^2\sin^2\phi)^{\frac{1}{4}}(1-\sqrt{k'})}, \frac{1-\sqrt{k'}}{\sqrt{(2+2k')}}\right\}\right.$$

$$\left. + \arg\operatorname{cn}\left\{\frac{(1-k^2\sin^2\phi)^{\frac{1}{2}}+\sqrt{k'}}{(1-k^2\sin^2\phi)^{\frac{1}{4}}(1+\sqrt{k'})}, \frac{1+\sqrt{k'}}{\sqrt{(2+2k')}}\right\}\right].$$

Let $$Z = \int_0^\phi \frac{d\phi}{(1-k^2\sin^2\phi)^{\frac{1}{3}}}$$

$$= \tfrac{3}{2}\int_x^1 \frac{xdx}{\sqrt{(1-x^3 \,.\, x^3-k'^2)}},$$

if $$x^3 = 1-k^2\sin^2\phi.$$

(Legendre, t. I. p. 180).

Put $$k' = n^3,$$

and $$x^2+n^2 = xz;$$

therefore $$x+n = x^{\frac{1}{2}}\sqrt{(z+2n)},$$

$$x-n = x^{\frac{1}{2}}\sqrt{(z-2n)}.$$

$$2x^{\frac{1}{2}} = \sqrt{(z+2n)}+\sqrt{(z-2n)},$$

$$2x^{-\frac{1}{2}}dx = \frac{dz}{\sqrt{(z+2n)}} + \frac{dz}{\sqrt{(z-2n)}}.$$

Therefore $$1-x^3\,.\,x^3-k'^2$$
$$=(1+n^6)\,x^3-x^6-n^6$$
$$=(1+n^6)\,x^3-(x^2+n^2)^3+3x^2n^2\,(x^2+n^2)$$
$$=(1+n^6)\,x^3-x^3z^3+3x^3n^2z,$$

and $$Z=\tfrac{3}{2}\int^1\frac{x^{-\frac{1}{2}}dx}{\sqrt{(1+n^6+3n^2z-z^3)}}$$

$$=\tfrac{3}{4}\int_z^{1+n^2}\frac{\dfrac{dz}{\sqrt{(z+2n)}}+\dfrac{dz}{\sqrt{(z-2n)}}}{\sqrt{(1+n^2-z)\left[\{z+\frac{1}{2}(1+n^2)^2\}+\frac{3}{4}(1-n^2)^2\right]}}$$

$$=\frac{3^{\frac{3}{4}}}{4\,(1+n+n^2)^{\frac{1}{4}}\,(1-n+n^2)^{\frac{3}{4}}}$$
$$\arg\operatorname{cn}\left\{\frac{(z+2n)\sqrt{3}\,(1+n+n^2)^{\frac{1}{2}}-(1+n^2-z)\,(1-n+n^2)^{\frac{1}{2}}}{(z+2n)\sqrt{3}\,(1+n+n^2)^{\frac{1}{2}}+(1+n^2-z)\,(1-n+n^2)^{\frac{1}{2}}},\ k_1\right\}$$

$$+\frac{3^{\frac{3}{4}}}{4\,(1+n+n^2)^{\frac{3}{4}}\,(1-n+n^2)^{\frac{1}{4}}}$$
$$\arg\operatorname{cn}\left\{\frac{(z+2n)\sqrt{3}\,(1-n+n^2)^{\frac{1}{2}}-(1+n^2-z)\,(1+n+n^2)^{\frac{1}{2}}}{(z+2n)\sqrt{3}\,(1-n+n^2)^{\frac{1}{2}}+(1+n^2-z)\,(1+n+n^2)^{\frac{1}{2}}},\ k_2\right\},$$

where
$$k_1^2=\tfrac{1}{2}\left\{1-\frac{\sqrt{3}}{2}\,\frac{(1-n+n^2)^2-3n^2}{(1+n+n^2)^{\frac{1}{2}}\,(1-n+n^2)^{\frac{3}{2}}}\right\},$$
$$k_2^2=\tfrac{1}{2}\left\{1-\frac{\sqrt{3}}{2}\,\frac{(1+n+n^2)^2-3n^2}{(1+n+n^2)^{\frac{3}{2}}\,(1-n+n^2)^{\frac{1}{2}}}\right\}$$

by (14).

Therefore $$\int_x^1\frac{x\,dx}{\sqrt{(1-x^3\,.\,x^3-k'^2)}}$$

$$=\frac{1}{2\sqrt[4]{3}\,(1+k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{4}}\,(1-k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{3}{4}}}$$
$$\arg\operatorname{cn}\left\{\frac{(x+k'^{\frac{1}{3}})^2\sqrt{3}\,(1+k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{2}}-(1-x)\,(x-k'^{\frac{1}{3}})\,(1-k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{2}}}{(x+k'^{\frac{1}{3}})^2\sqrt{3}\,(1+k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{2}}+(1-x)\,(x-k'^{\frac{1}{3}})\,(1-k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{2}}},\ k_1\right\}$$

$$+\frac{1}{2\sqrt[4]{3}\,(1+k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{3}{4}}\,(1-k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{4}}}$$
$$\arg\operatorname{cn}\left\{\frac{(x+k'^{\frac{1}{3}})^2\sqrt{3}\,(1-k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{2}}-(1-x)\,(x-k'^{\frac{1}{3}})\,(1+k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{2}}}{(x+k'^{\frac{1}{3}})^2\sqrt{3}\,(1-k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{2}}+(1-x)\,(x-k'^{\frac{1}{3}})\,(1+k'^{\frac{1}{3}}+k'^{\frac{2}{3}})^{\frac{1}{2}}},\ k_2\right\}$$

and
$$\int_0^\phi \frac{d\phi}{(1-k^2\sin^2\phi)^{\frac{1}{3}}}$$

is obtained by writing $(1-k^2\sin^2\phi)^{\frac{1}{3}}$ for $x$ in the preceding expression.

Similarly
$$\int_0^\phi \frac{d\phi}{(1-k^2\sin^2\phi)^{\frac{2}{3}}}$$

$$= \tfrac{3}{2}\int_x^1 \frac{dx}{\sqrt{(1-x^3 \,.\, x^3-k'^2)}}$$

$$= \tfrac{3}{2}\int_x^1 \frac{x^{-\frac{3}{2}}\,dx}{\sqrt{(1+n^6+3n^2z-z^3)}}$$

$$= \frac{3}{4n}\int_z^{1+n^2} \frac{\dfrac{dz}{\sqrt{z-2n}}-\dfrac{dz}{\sqrt{z+2n}}}{\sqrt{(1+n^2-z)\left[\{z+\frac{1}{2}(1+n^2)\}^2+\frac{3}{4}(1-n^2)^2\right]}}$$

can be written down.

Many more examples of the same kind can be collected from the papers of L. Königsberger on the "Reduction of Abel's integrals to elliptic and hyperelliptic integrals," in *Crelle* and the *Mathematische Annalen.*

(5) H. Middleton, *On a method of constructing an electrical telescope, and the application of the principle to the theory of vision.*

*April* 19, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

Asa Gray, Professor of Natural History in Harvard University, Cambridge, Mass., having been nominated by the Council, was ballotted for and duly elected an honorary member of the Society.

W. W. Cordeaux, St John's College, and C. M. Prior, Trinity Hall, were ballotted for and duly elected Associates of the Society.

The following communications were made to the Society:—

(1) LORD RAYLEIGH, M.A., *On the minimum aberration of a single lens for parallel rays.*

It is well known that when the material of a lens is plate glass ($\mu = 1{\cdot}5$), the aberration is least when the lens is double convex, the radius of the anterior surface $r$ being equal to $\frac{7}{12}$ of the focal length $f$, and that of the posterior surface $(-s)$ equal to $\frac{7}{2}f$. The residual aberration $\delta f$ is then given by

$$\delta f = -\tfrac{15}{14}\frac{y^2}{f} \quad\text{..........................(1)},$$

$y$ being the semiaperture *.

In the older works on Optics the special supposition that $\mu = 1{\cdot}5$ is introduced at the beginning of the calculations, so that the results are not available for an examination of the effect of a varying refractive index; but it has been repeatedly asserted that lenses formed of diamonds or of other precious stones of high refracting power have an almost inappreciable aberration †. In Coddington's *Optics*, § 89, the minimum aberration for $\mu = 2$ is stated to be only $\frac{1}{16}\frac{y^2}{f}$, but no algebraical calculation is given.

* Parkinson's *Optics*, §§ 130, 131.
† E.g. *Optics. Encyclopædia Britannica*, 1842.

The general expression of the aberration for parallel rays is *

$$-\delta f : \frac{y^2}{f} = \frac{\mu - 1}{2\mu^2} \left\{ \frac{1}{r^3} + \left( \frac{\mu + 1}{f} - \frac{1}{s} \right) \left( \frac{1}{f} - \frac{1}{s} \right)^2 \right\} f^3 \ldots\ldots (2),$$

while $r$, $s$, and $f$ are connected by

$$\frac{1}{f} = (\mu - 1) \left( \frac{1}{r} - \frac{1}{s} \right) \ldots\ldots\ldots\ldots\ldots\ldots (3).$$

Writing for brevity $R$, $S$, $F$ respectively for $r^{-1}$, $s^{-1}$, $f^{-1}$, and taking $G = \frac{\mu F}{\mu - 1}$, so that $-S = \frac{G}{\mu} - R$, we get

$$\frac{1}{r^3} + \left( \frac{\mu + 1}{f} - \frac{1}{s} \right) \left( \frac{1}{f} - \frac{1}{s} \right)^2$$

$$= G \left\{ R^2 (\mu + 2) - RG (2\mu + 1) + \mu G^2 \right\}$$

$$= G \left\{ \sqrt{(\mu + 2)} \, . \, R - \frac{(2\mu + 1) \, G}{2 \sqrt{(\mu + 2)}} \right\}^2 + \frac{\mu^3 F^3}{(\mu - 1)^3} \, \frac{4\mu - 1}{4 \, (\mu + 2)} \ldots (4).$$

Since $\mu > 1$, both terms are of the same sign, and the aberration can never vanish. If $f$ and $y$ be given, the aberration is least when

$$\sqrt{(\mu + 2)} \, . \, R = \frac{2\mu + 1}{2 \sqrt{(\mu + 2)}} \, . \, G,$$

that is, when

$$r = \frac{2 \, (\mu + 2) \, (\mu - 1)}{\mu \, (2\mu + 1)} f \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (5).$$

The corresponding value of $s$ is

$$-s = \frac{2 \, (\mu + 2) \, (\mu - 1)}{4 + \mu - 2\mu^2} f \ldots\ldots\ldots\ldots\ldots\ldots (6),$$

so that

$$-s : r = \frac{\mu \, (2\mu + 1)}{4 + \mu - 2\mu^2} \ldots\ldots\ldots\ldots\ldots\ldots (7),$$

which agrees with the result of § 130.

When this condition is satisfied, the second term of (4) gives for the minimum aberration

$$-\delta f : \frac{y^2}{f} = \frac{\mu \, (4\mu - 1)}{8 \, (\mu - 1)^2 \, (\mu + 2)} \ldots\ldots\ldots\ldots\ldots (8),$$

which is applicable to all values of $\mu$.

* Parkinson's *Optics*, § 129.

If $\mu = 2$, (8) gives

$$-\delta f : \frac{y^2}{f} = \tfrac{7}{16},$$

not $\frac{1}{16}$, as stated by Coddington. The aberration tends indeed to become less as $\mu$ increases, but it remains considerable for all substances known in nature.

It seems to have been thought evident that great advantage would result from higher refracting power on account of its allowing the use of more moderate curvatures. It appears however from (5) and (6) that as $\mu$ increases, $r$ and $s$ do not tend to become infinite for the form of minimum aberration, but approach the finite value $f$.

(2) A. FREEMAN, M.A., *Note on the value of the least root of an equation allied to $J_0(z) = 0$.*

The equation in question, viz.

$$1 - \frac{x}{1^2} + \frac{x^2}{1^2 . 2^2} - \frac{x^3}{1^2 . 2^2 . 3^2} + \frac{x^4}{1^2 . 2^2 . 3^2 . 4^2} - \&c. = 0 \text{ .......(1)}$$

is identical with $J_0(z) = 0$ if we write $\frac{z^2}{2^2}$ for $x$.

(See Lommel's *Bessel'schen Functionen*, page 26.)

In a memoir on the Calculus of Variations printed in the twelfth volume of *Mémoires de l'Académie des Sciences*, Paris, 1833, Poisson had occasion for the least root of the equation above given. He seems to have applied to M. Largeteau of the *Bureau des Longitudes*, by whom the calculation was made, and Poisson quotes the number thus: 1·46796491.

The equation occurs in the problem of the flow of heat in an infinite cylinder, and in my edition of Fourier's *Théorie Analytique de la Chaleur* I gave, in a note, page 310, the result of an approximation, including terms of the series as far as $x^4$; my result was 1·4467.

Suspecting some error in the Poisson-Largeteau number, I first supposed it might need only the insertion of an additional figure 4 after the decimal point, taking it to have really been 1·446796491.

On substituting this value of $x$ in the first five terms of the series, the algebraic sum became $-$ 0·000009, but the value of the fifth term was as large as 0·007607.

It was therefore obvious that no more than five terms of the series had been employed in M. Largeteau's calculation. It seemed desirable to push my own approximation further, and after some trouble I found 1·4458 to be a much closer approach to the root. This value of $x$ makes the algebraic sum of the first eight terms of the series to be 0·0000093.

Proceeding then from the number 1·4458 by Newton's method of approximation applied to the first eight terms of the series I at once found the further approximate value of the root to be 1·4457963.

By way of verification I calculated the value of the first eight terms of the series corresponding to this number, and can write down the results thus:

$$\begin{array}{r|r r|r} + & 1\cdot0000000 & - & 1\cdot4457963 \\ & \cdot5225817 & & \cdot0839496 \\ & \cdot0075859 & & \cdot0004387 \\ & \cdot0000176 & & \cdot0000005 \\ \hline = + & 1\cdot5301852 & - & 1\cdot5301851 \end{array}$$

$= 0·0000001$, the corresponding sum of the series.

It will be noticed that the value of the eighth term is numerically only a 5 in the seventh place of decimals; hence I consider that we can with confidence rely on the value of the least root of this important equation being very accurately expressed by the number above given, namely 1·4457963.

I may remark that the table in Lommel's *Bessel'schen Functionen* is not sufficiently extensive to be of much use in the foregoing investigation.

But there is a table of the first ten roots of the equation

$$1 - \frac{x^2}{2^2} + \frac{x^4}{2^2 \,.\, 4^2} - \frac{x^6}{2^2 \,.\, 4^2 \,.\, 6^2} + \&c. = 0,$$

that is $J_0(x) = 0$, given on p. 186 of a memoir by Professor Stokes, *On the numerical calculation of a class of definite integrals and infinite series* (*Camb. Phil. Trans.*, Vol. IX.).

We have there given the first root $(0·7655)\pi$.

Now $$\{\tfrac{1}{2}(0·7655\,\pi)\}^2 = 1·4458,$$

which is the figure from which my present approximation proceeds. This first root was obtained by Professor Stokes by interpolation from a table given by Sir G. B. Airy. The remaining part of the table was calculated by methods given in the memoir itself.

Lord Rayleigh in his paper *On the numerical calculation of the roots of fluctuating functions* (*Proc. London Math. Soc.*, Vol. v. p. 122) has calculated the least root of the equation $J_0(z) = 0$, his result being 2·404826. Denoting this by $z_1$, the least root of the equation (1)

$$= (\tfrac{1}{2}z_1)^2 = (1{\cdot}202413)^2 = 1{\cdot}445797,$$

which confirms the value found above.

*May* 3, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

E. B. Tawney, M.A., Trinity College, was ballotted for and duly elected a Fellow of this Society.

The following communications were made to the Society:—

(1) W. KEEPING, M.A., *On the included pebbles of the Neocomian deposits of Potton and Upware, and their bearing upon the physical features of the lower cretaceous period.*

The Lower Greensand (Upper Neocomian) beds beneath the Gault at Potton in Bedfordshire, Upware near Cambridge, and elsewhere, include various pebble beds and conglomerates. The majority of the pebbles are phosphatic nodules or "coprolites," for which the beds have been worked during the last ten or twelve years. These were all of them derived as pebbles from older Neocomian and Jurassic rocks, and saturated with phosphate during the formation of the Upware and Potton beds. Besides these there are many other pebbles which are not phosphatised, and which are therefore picked out by the work-people and accumulated in rubbish heaps at the diggings. They belong to various ages, namely: I. Lower Cretaceous; II. Jurassic (impure limestone, chert, and sandstone); and III. more ancient rock fragments. These latter are considered under two first headings according to their sizes. The smaller pebbles, which make up regular conglomerates, consist of quartz, apparently vein quartz, and subangular fragments of hard, highly silicious, fine-grained rocks, many of which are chert, the rest being highly indurated argillites (Lydian stone, etc.). The larger pebbles (diameter more than an inch) are vein quartz, vein breccia, quartzite, altered sandstone, probably of carboniferous age, and altered grits.

Amongst the more interesting pebbles are (*a*) the angular and subangular fragments of indurated shale and Lydian stone; (*b*) a very angular, joint-hacked piece of Cambrian or Silurian pale slate, in which a group of fossils was found; and (*c*) the masses of

chert, also frequently angular and subangular, and of various colours. These belong to two separate ages, namely: (i) Mountain Limestone chert, some of it very flinty and transparent, and containing a number of characteristic fossils; and (ii) a more opaque and reddish chert with Jurassic fossils. Also (*d*) a pebble of "devitrified pitch-stone" described by Professor Bonney, who recognizes it as very similar to the ancient Rhyolite lavas of the Wrekin.

With regard to the original homes of the pebbles the great majority of the phosphatised fragments were derived as pebbles from the ancient cliffs of Jurassic rocks which were being denuded away around Cambridge and Potton along the old shore-line of the Upper Neocomian sea. The new Red Sandstone and older Jurassic rocks of the Midlands probably also supplied some of the pebbles. But, having restored the ancient land surfaces of the period and regarded this as a source of pebble supply, it is found that the small and distant exposures of carboniferous and older Palæozoic rocks in Derbyshire and Charnwood Forest are utterly inadequate, and we are compelled to look elsewhere for their origin. From the angularity of the majority of the groups of pebbles and from their dissimilarity to the pebbles in older conglomerates, it is clear that the pebbles must have been derived *directly* from the parent rock, and not handed on from older pebble beds.

The ridge of Palæozoic rocks known as the Harwich axis formed, in earlier Neocomian times, a barrier of land separating the northern (or Anglo-Germanic) from the southern (or Anglo-Gallic) sea; but gradually it subsided, suffering great denudation from the work of waves and currents, as it became slowly submerged. Ultimately the northern and southern seas met and surged together through narrow channels, and these were gradually widened, over Cambridgeshire, until the barrier totally disappeared under water. In this destruction of the old barrier we find an abundant source of Palæozoic pebbles. Recent deep borings have discovered the nature of the old axis rocks at Turnford (Devonian) and at Ware (Silurian), and the relations of these are such as to lead us to expect the occurrence of still older Palæozoic beds further north, near Cambridge. Thus the details of the structure of the ancient barrier, so far as yet known, support the independent conclusion that the majority of the older Palæozoic pebbles of our Neocomian pebble-beds were derived from this source.

The pebbles of chert point to the former existence of Jurassic limestone over this area; but no very satisfactory evidence appears bearing upon the question whether coal-bearing beds also form part of this old Palæozoic axis.

(2) ADAM SEDGWICK, B.A., *On the development of the structure known as the 'glomerulus of the head-kidney' in the chick.*

In a paper by Mr Balfour and myself in the *Quart. Journ. of Micr. Science*, Vol. XIX., describing the development of what we believed to be a rudimentary head-kidney in the chick, we drew attention to a structure which so closely resembled the glomerulus of the head-kidney of the Icthyopsida that we identified it as an homologous structure.

Gasser[1] has also independently discovered and similarly identified this structure.

In the paper just referred to no attempt was made to trace the development of this glomerulus, but it was merely described as it appeared at its time of greatest development.

The following description is taken from that paper:

"In the chick the glomerulus is paired, and consists of a vascular outgrowth or ridge projecting into the body cavity on each side at the root of the mesentery. It extends from the anterior end of the Wolffian body to the point where the foremost opening of the head-kidney commences. We have found it at a period slightly earlier than that of the first development of the head-kidney....In the interior of this body is seen a stroma with numerous vascular channels and blood corpuscles, and a vascular connection is apparently becoming established, if it is not so already, between the glomerulus and the aorta. The stalk connecting the glomerulus with the attachment of the mesentery varies in thickness in different sections, but we believe that the glomerulus is continued unbroken throughout the very considerable region through which it extends. This point is, however, difficult to make sure of, owing to the facility with which the glomerulus breaks away. At the stage we are describing no true Malpighian bodies are present in the part of the Wolffian body on the same level with the anterior end of the glomerulus, but the Wolffian body merely consists of the Wolffian duct. At the level of the posterior part of the glomerulus this is no longer the case, but here a regular series of primary Malpighian bodies is present, and the glomerulus of the head-kidney may frequently be seen in the same section as a Malpighian body. In most sections the two bodies appear quite disconnected, but in those sections in which the glomerulus of the Malpighian body comes into view it is seen to be derived from the same formation as the glomerulus of the head-kidney."

The point which is left in doubt in the above description, viz. as to whether the glomerulus constitutes a continuous structure, is at once decided by a study of its development.

[1] *Sitzungsberichte der Gesellschaft zur Beförd d. gesam. Naturwiss.*, No. 5, 1879.

I may here state that it is not a continuous structure, but consists of a series of external glomeruli, each of which corresponds and is continuous with the glomeruli of the Malpighian bodies found in this part of the trunk.

I will commence the description of the development at the time when the segmental tubes have reached the stage of development figured by Kölliker[1] and myself[2]. At this stage each of them in the anterior region of the Wolffian body has the form of an **S**-shaped string, with a narrow opening into the body cavity, the lower limb of the **S** being formed by the intermediate cell mass, and the upper limb by a column of cells which connects the intermediate cell mass with the Wolffian duct.

In the region where each external glomerulus is afterwards found the openings into the body cavity which are homologous with the peritoneal openings of the segmental tubes in Elasmobranchs widen out very considerably, and a lumen is continued from them into the intermediate cell mass on the one hand, and on the other hand into the column of cells which forms the upper limb of the **S** and connects the intermediate cell mass with the Wolffian duct[3].

That part of the segmental tube which will afterwards become a Malpighian body is therefore in the region where an external glomerulus will subsequently be formed connected with the body cavity by a short tube. This tube rapidly widens out, especially anteriorly, to such an extent that it soon appears as a shallow bay in the body cavity. Thus each opening at this stage forms a bay, wide and shallow anteriorly, becoming deeper and narrower as we pass backwards, until finally behind it is separated from the body cavity altogether, and there is seen in section a Malpighian capsule precisely resembling a developing Malpighian capsule in the hinder region of the Wolffian body[4]. In this bay and in the small part behind continuous with the bay, but separated from the body cavity, which are together serially homologous with a Malpighian capsule and the funnel leading from it into the body cavity, a small glomerulus soon appears attached to the dorsal wall.

[1] *Entwicklungsgeschichte des Menschen u. der höheren Thiere*, p. 201, 2nd ed.

[2] *Quart. Journ. Mic. Sc.*, April, 1880, pl. XVII., fig. 1.

[3] This may best be understood by examining fig. 11, pl. XVII. in my paper already referred to (*Q. J. M. S.*, April, 1880). If the primary Wolffian tubule ($wt^1$) here represented were connected with the peritoneal epithelium at the point where the line from $wt^1$ cuts it, and it were open to the body cavity at that point, an appearance similar to that which I have attempted to describe would be obtained. Or perhaps a better idea of the structure may be obtained from fig. 6, pl. XX. in Balfour's *Monograph on the Development of Elasmobranch Fishes.* If *st* were very short and wide, so that *mg* were widely open to the body cavity, the figure would resemble a developing Wolffian tubule in this anterior part of the chick's Wolffian body.

[4] *loc. cit.* fig. 11.

The glomerulus increases in size, and the bay anteriorly widens out very much, while behind it remains deep, and finally passes into the closed posterior portion. The glomerulus fills up this passage which clearly runs obliquely backwards and dorsalwards, and eventually, as far as I can ascertain, the opening becomes completely closed, the epithelium on the external glomerulus being no longer continued through the opening on to the internal glomerulus.

The external glomerulus, then, in the chick which has hitherto been known as the glomerulus of the head-kidney, is nothing more than a series of glomeruli of primary Malpighian bodies projecting through the wide openings of the segmental tubes into the body cavity. Their extreme antero-posterior extension may be said to be within the 9th and 13th segments.

In the chick the primary segmental tubes corresponding to these external glomeruli are apparently never fully developed.

I may mention that the external glomeruli are present in greater numbers and attain a greater development in the duck than in the chick.

I defer the details and all discussion of this extraordinary and unexpected development until I am able to publish a fuller paper with figures.

*May* 17, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:—

(1) C. TAYLOR, M.A., *On Newton's organic description of curves.*

1. *If two angles of given magnitudes DBM, DCM turn about their summits B and C as poles, whilst the intersection M moves along a fixed straight line or directrix, the remaining intersection D traces a conic passing through B and C.*

The above is well known as Newton's method of generating conic sections, but it is not so generally known that he also extended his organic description to the higher curves. M. Chasles, in his *Aperçu historique*, &c., Note xv., p. 337 (2me ed., 1875), first extends the theorem above stated by supposing the point $M$ to move upon a conic through $B$ and $C$ (instead of a straight line), in which case also $D$ traces a conic through $B$ and $C$ as before. He then goes on to remark:—

"Ce théorème, qui est une généralisation de celui de Newton, n'est lui-même qu'une manière particulière, entre une infinité d'autres semblables, pour former les coniques par l'intersection de deux droites mobiles autour de deux points fixes, &c. Ainsi le théorème de Newton, qui a eu quelque célébrité, et qui a paru capital dans la théorie des coniques, ne se trouve plus qu'un cas très-particulier d'un mode général de description de ces courbes."

But in the first place the particular "généralisation" of Newton's theorem here referred to, and elsewhere cited as Chasles' extension of Newton's theorem, is itself only "un cas très-particulier" of Newton's *Curvarum Descriptio Organica**. And in the next place this extension would have served no useful purpose in the *Principia*, where the primary theorem is given†. Newton is there dealing with the determination of orbits from given conditions, and his construction enables him to determine a conic or orbit of which five points are given. Chasles' extension only enables us to draw a conic by supposing a conic to be already drawn.

2. Newton's *Enumeratio linearum Tertii Ordinis*, first published as an appendix to his *Opticks* (1704), contains a general statement of his *Theoremata de Curvarum descriptione organica* (§ XXXI.). In this tract he enunciates the following propositions:—

If two angles of given magnitudes $PAD$ and $PBD$ turn about $A$ and $B$ as poles given in position, then if the intersection $P$ of one pair of their arms be made to describe a conic, the intersection $D$ of the other pair will in general describe a curve of the third *genus* [or fourth *order*] having double points at $A$ and $B$ and at the limiting position of $D$ where the angles $BAP$ and $ABP$ vanish together: but the locus of $D$ will be of the second genus if the angles $BAD$ and $ABD$ vanish together. If $P$ describes a conic passing through $A$, then $D$ describes a cubic having a double point at $A$ and passing through $B$.

The well-known case in which the locus is a conic is again stated, and the cases in which it degenerates into a straight line are pointed out. It is further evident that when $P$ describes a conic through both $A$ and $B$, the cubic, the locus of $D$, has *two* double points and must therefore degenerate. Thus the so-called extension of Newton's organic description is seen to be a special case of it.

3. A double point $A$ and six other points $BCDEFG$ of a cubic being given, the curve is organically described as follows: Draw

* The *Aperçu historique* does contain a reference to this (p. 145) but in the above-mentioned Note xv., which must have misled many readers, it is overlooked.

† *Principia*, Lib. I. sect. 5, lemma 21. See also *Arithmetica Universalis*, prob. 53 (1707).

the triangle $ABC$, and then make the angle $CAB$ turn about $A$, and the angle $ABC$ about $B$; and let $C'$ denote the point of concourse initially at $C$, and $K$ the point of concourse of the other two arms of the rotating angles. Now let $C'$ coincide successively with $DEFG$, and let the corresponding positions of $K$ be $PQRS$. Describe the conic $APQRS$ by the organic method, and let $K$ move on the conic thus described; then $C'$ describes the cubic as required. See § XXXIII. p. [159].

4. Newton further remarks that curves of the higher orders, having a double point, may be described in like manner. He also points out that one finite angle and a vanishing angle (or *straight line*) may be used in the *Descriptio Organica*, in place of two finite angles. The full development of what he has there briefly stated may be found in Maclaurin's *Geometria Organica* (1720).

(2) Mr J. W. L. GLAISHER. *Addition to a previous paper on some theorems in trigonometry.*

The following is an addition to my paper on pp. 319—329.

§ 12. *Theorem.* If we have, identically,

$$\Sigma\,.\,\Pi \sin(\alpha-\beta)=0 \text{.....................(1)},$$

where $\Pi \sin(\alpha-\beta)$ denotes a product of sines of *differences* of angles, and $\Sigma$ denotes the sum of any number of such products, each of which however must contain the same number of sines, then

$$\Sigma\,.\,\Pi \sin(\alpha-\beta)\sin(\alpha+\beta)=0.$$

The proof of the theorem is very simple for in virtue of (1), by expanding the sines and equating the terms of lowest dimensions,

$$\Sigma\,.\,\Pi(\alpha-\beta)=0,$$

whence, writing in this identity $\alpha^2$, $\beta^2$, ... for $\alpha$, $\beta$, ...

$$\Sigma\,.\,\Pi(\alpha^2-\beta^2)=0,$$

and replacing $\alpha$, $\beta$, ... by $\sin\alpha$, $\sin\beta$, ... the identity becomes

$$\Sigma\,.\,\Pi(\sin^2\alpha-\sin^2\beta)=0,$$

that is
$$\Sigma\,.\,\Pi \sin(\alpha-\beta)\sin(\alpha+\beta)=0.$$

§ 13. As an example, take the well-known identity

$$\begin{aligned}&\sin(\beta-\gamma)\sin(\alpha-\delta)\\&+\sin(\gamma-\alpha)\sin(\beta-\delta)\\&+\sin(\alpha-\beta)\sin(\gamma-\delta)=0 \text{.................(2)};\end{aligned}$$

this, by the theorem, gives

$$\begin{aligned}&\sin(\beta-\gamma)\sin(\alpha-\delta)\sin(\beta+\gamma)\sin(\alpha+\delta)\\&+\sin(\gamma-\alpha)\sin(\beta-\delta)\sin(\gamma+\alpha)\sin(\beta+\delta)\\&+\sin(\alpha-\beta)\sin(\gamma-\delta)\sin(\alpha+\beta)\sin(\gamma+\delta)=0\ldots\ldots(3),\end{aligned}$$

and the proof is, that from (2) we have

$$(\beta-\gamma)(\alpha-\delta)+(\gamma-\alpha)(\beta-\delta)+(\alpha-\beta)(\gamma-\delta)=0,$$

identically, and therefore

$$(\beta^2-\gamma^2)(\alpha^2-\delta^2)+(\gamma^2-\alpha^2)(\beta^2-\delta^2)+(\alpha^2-\beta^2)(\gamma^2-\delta^2)=0,$$

identically; and this last equation on substituting $\sin\alpha$, $\sin\beta$, $\sin\gamma$, $\sin\delta$ for $\alpha$, $\beta$, $\gamma$, $\delta$ becomes

$$\begin{aligned}(\sin^2\beta-\sin^2\gamma)(\sin^2\alpha-\sin^2\delta)+(\sin^2\gamma-\sin^2\alpha)(\sin^2\beta-\sin^2\delta)&\\+(\sin^2\alpha-\sin^2\beta)(\sin^2\gamma-\sin^2\delta)&=0,\end{aligned}$$

which is (3).

The equation (3) is in fact ($B$) of § 7 (p. 323) in the form ($B'$), (p. 324); and it thus appears that this formula ($B$), viz.

$$\Pi(\sin a)=\Pi(\sin a')+\Pi(\sin a'')$$

is an immediate corollary from (2).

§ 14. As another example of the theorem in § 12, take the formula

$$\begin{aligned}&\sin(\beta-\gamma)\sin(\beta-\delta)\sin(\gamma-\delta)\\&+\sin(\gamma-\alpha)\sin(\gamma-\delta)\sin(\alpha-\delta)\\&+\sin(\alpha-\beta)\sin(\alpha-\delta)\sin(\beta-\delta)\\&+\sin(\beta-\gamma)\sin(\gamma-\alpha)\sin(\alpha-\beta)=0,\end{aligned}$$

which gives

$$\begin{aligned}&\sin(\beta-\gamma)\sin(\beta-\delta)\sin(\gamma-\delta)\sin(\beta+\gamma)\sin(\beta+\delta)\sin(\gamma+\delta)\\&+\sin(\gamma-\alpha)\sin(\gamma-\delta)\sin(\alpha-\delta)\sin(\gamma+\alpha)\sin(\gamma+\delta)\sin(\alpha+\delta)\\&+\sin(\alpha-\beta)\sin(\alpha-\delta)\sin(\beta-\delta)\sin(\alpha+\beta)\sin(\alpha+\delta)\sin(\beta+\delta)\\&+\sin(\beta-\gamma)\sin(\gamma-\alpha)\sin(\alpha-\beta)\sin(\beta+\gamma)\sin(\gamma+\alpha)\sin(\alpha+\beta)\\&=0.\end{aligned}$$

This equation may, in the notation of § 7, be written

$$\begin{aligned}&\sin a\sin b\,(\sin a'\sin b'\sin a''\sin b''+\sin c'\sin d'\sin c''\sin d'')\\&=\sin c\sin d\,(\sin a'\sin b'\sin c''\sin d''+\sin a''\sin b''\sin c'\sin d').\end{aligned}$$

§ 15. If $(a')'$, $(b')'$, $(c')'$, $(d')'$ denote the quantities formed from $a'$, $b'$, $c'$, $d'$ by the same process as $a'$, $b'$, $c'$, $d'$ were formed from $a$, $b$, $c$, $d$, and if a corresponding meaning be assigned to $(a')''$, ... $(a'')'$, ... $(a'')''$, ... then it can be shown that we have the four sets of equations

$$\begin{array}{llll}(a')'=a, & (a'')''=a, & (a'')'=-a', & (a')''=\ \ a'',\\ (b')'=b, & (b'')''=b, & (b'')'=\ \ b', & (b')''=-b'',\\ (c')'=c, & (c'')''=c, & (c'')'=\ \ c', & (c')''=-c'',\\ (d')'=d, & (d'')''=d, & (d'')'=\ \ d', & (d')''=-d''.\end{array}$$

§ 16. Taking the known formulæ

$$\begin{aligned}8\cos a\cos b\cos c\cos d &= \cos 2a' + \cos 2b' + \cos 2c' + \cos 2d'\\ &\quad + \cos 2a'' + \cos 2b'' + \cos 2c'' + \cos 2d'',\\ 8\sin a\sin b\sin c\sin d &= -\cos 2a' - \cos 2b' - \cos 2c' - \cos 2d'\\ &\quad + \cos 2a'' + \cos 2b'' + \cos 2c'' + \cos 2d'',\end{aligned}$$

which may be written

$$\begin{aligned}8\,\Pi(\cos a) &= \Sigma\cos 2a' + \Sigma\cos 2a'' \ldots\ldots\ldots\ldots\text{(i)},\\ 8\,\Pi(\sin a) &= -\Sigma\cos 2a' + \Sigma\cos 2a'' \ldots\ldots\ldots\ldots\text{(ii)},\end{aligned}$$

we obtain from them by subtraction and addition the formulæ

$$\begin{aligned}4\{\Pi(\cos a) - \Pi(\sin a)\} &= \Sigma\cos 2a',\\ 4\{\Pi(\cos a) + \Pi(\sin a)\} &= \Sigma\cos 2a'',\end{aligned}$$

the former of which is $(E)$, (§ 7, p. 323).

Substituting $a'$, $b'$, ... and $a''$, $b''$, ... respectively in place of $a$, $b$, ... in (i) and (ii) we have in virtue of the equations in § 15,

$$\begin{aligned}8\Pi(\cos a') &= \Sigma\cos 2a + \Sigma\cos 2a'' \ldots\ldots\ldots\ldots\text{(iii)},\\ 8\Pi(\sin a') &= -\Sigma\cos 2a + \Sigma\cos 2a'' \ldots\ldots\ldots\ldots\text{(iv)},\\ 8\Pi(\cos a'') &= \Sigma\cos 2a' + \Sigma\cos 2a \ldots\ldots\ldots\ldots\text{(v)},\\ 8\Pi(\sin a'') &= -\Sigma\cos 2a' + \Sigma\cos 2a \ldots\ldots\ldots\ldots\text{(vi)},\end{aligned}$$

and writing for the moment $(2a')$, $(2a'')$, ... in place of $\Sigma\cos 2a$, $\Sigma\cos 2a'$, ... the formulæ (i), ... (vi) are

$$\begin{array}{ll}8\Pi(\cos a) = (2a') + (2a''), & 8\Pi(\sin a) = -(2a') + (2a''),\\ 8\Pi(\cos a') = (2a'') + (2a), & 8\Pi(\sin a') = (2a'') - (2a),\\ 8\Pi(\cos a'') = (2a) + (2a'), & 8\Pi(\sin a'') = (2a) - (2a'),\end{array}$$

from which we deduce that

$$\begin{aligned}\Pi(\sin a) &= \Pi(\sin a') + \Pi(\sin a''),\\ \Pi(\cos a) &= \Pi(\cos a') - \Pi(\sin a''),\end{aligned}$$

which are $(B)$ and $(C)$ of § 7. The equation (vi) is in fact $(D)$, and $(A)$ is deducible by addition from $(B)$ and $(C)$, so that we have thus obtained the five theorems $(A)$, ... $(E)$ of § 7.*

§ 17. The formula $(B)$, viz. $\Pi(\sin a) = \Pi(\sin a') + \Pi(\sin a'')$, is in fact a particular case of a more general theorem, connecting products of four sines, which may be written

$$\begin{aligned}
&\sin(a-f)\sin(a-g)\sin(a-h)\sin(b-c)\\
&+\sin(b-f)\sin(b-g)\sin(b-h)\sin(c-a)\\
&+\sin(c-f)\sin(c-g)\sin(c-h)\sin(a-b)\\
&+\sin(b-c)\sin(c-a)\sin(a-b)\sin(a+b+c-f-g-h)=0\ldots(p),
\end{aligned}$$

where $a, b, c, f, g, h$ are any six quantities. The formula $(p)$ however involves only five independent quantities.

If we put $\qquad g+h=0, \qquad a+b+c=f,$

so that the last term vanishes, then $(p)$ becomes

$$\begin{aligned}
&\sin(b+c)\sin(a-g)\sin(a+g)\sin(b-c)\\
&+\sin(c+a)\sin(b-g)\sin(b+g)\sin(c-a)\\
&+\sin(a+b)\sin(c-g)\sin(c+g)\sin(a-b)=0,
\end{aligned}$$

which is in fact $(B)$ in the form $(B')$, (§ 8).

§ 18. The formula $(p)$ is in effect due to Professor Cayley and Mr R. F. Scott. In the *Messenger of Mathematics*, vol. V., p. 164, Professor Cayley gave the theorem: if

$$A+B+C+F+G+H=0,$$

then
$$\begin{vmatrix}
\sin(A+F)\sin(B+F)\sin(C+F), & \cos F, & \sin F\\
\sin(A+G)\sin(B+G)\sin(C+G), & \cos G, & \sin G\\
\sin(A+H)\sin(B+H)\sin(C+H), & \cos H, & \sin H
\end{vmatrix}=0,$$

and in the *Messenger*, vol. VIII., p. 155, Mr Scott evaluated this determinant, and showed that, the letters being unrestricted, it was equal to

$$\sin(G-H)\sin(H-F)\sin(F-G)\sin(A+B+C+F+G+H).$$

On expanding the determinant, and replacing $A, B, C, F, G, H$ by $-f, -g, -h, a, b, c$ respectively, we obtain the equation $(p)$.

* Since this paper was read to the Society some additional examples, &c. in connexion with §§ 12—16 have been published in the *Messenger of Mathematics*, vol. X. pp. 26—34 (June and July, 1880).

§ 19. Since $\sin(a-f)=\frac{1}{2i}\left(\frac{e^{ai}}{e^{fi}}-\frac{e^{fi}}{e^{ai}}\right)$, &c., it is evident that ($p$) is equivalent to the algebraical theorem

$$\begin{aligned}&\left(\frac{a}{f}-\frac{f}{a}\right)\left(\frac{a}{g}-\frac{g}{a}\right)\left(\frac{a}{h}-\frac{h}{a}\right)\left(\frac{b}{c}-\frac{c}{b}\right)\\+&\left(\frac{b}{f}-\frac{f}{b}\right)\left(\frac{b}{g}-\frac{g}{b}\right)\left(\frac{b}{h}-\frac{h}{b}\right)\left(\frac{c}{a}-\frac{a}{c}\right)\\+&\left(\frac{c}{f}-\frac{f}{c}\right)\left(\frac{c}{g}-\frac{g}{c}\right)\left(\frac{c}{h}-\frac{h}{c}\right)\left(\frac{a}{b}-\frac{b}{a}\right)\\+&\left(\frac{b}{c}-\frac{c}{b}\right)\left(\frac{c}{a}-\frac{a}{c}\right)\left(\frac{a}{b}-\frac{b}{a}\right)\left(\frac{abc}{fgh}-\frac{fgh}{abc}\right)=0,\end{aligned}$$

which may be readily verified. This process of substitution of algebraical expressions for the sines (which is that employed by Mr Scott in evaluating the determinant) affords perhaps the simplest demonstration of ($p$).

§ 20. By putting $a, b, \ldots$ in place of $a^2, b^2, \ldots$ in the equation contained in the last section, and by expanding the sines in ($p$) and equating the terms of the fourth order, we have the following pair of algebraical identities,

$$\begin{aligned}&bc(b-c)(a-f)(a-g)(a-h)\\+&ca(c-a)(b-f)(b-g)(b-h)\\+&ab(a-b)(c-f)(c-g)(c-h)\\+&(b-c)(c-a)(a-b)(abc-fgh)=0,\end{aligned}$$

$$\begin{aligned}&(b-c)(a-f)(a-g)(a-h)\\+&(c-a)(b-f)(b-g)(b-h)\\+&(a-b)(c-f)(c-g)(c-h)\\+&(b-c)(c-a)(a-b)(a+b+c-f-g-h)=0.\end{aligned}$$

It is interesting to notice the manner in which the trigonometrical theorem establishes, as it were, a connection between these identities. They can of course be easily verified.

*Addition to* Mr HICKS' *paper on the problem of two pulsating spheres in a fluid* (pp. 276—285):

On page 279 of the last part of the *Proceedings* it is stated "we know that $V^2$ has no effect on the resultant force." This is of course only true of the resultant force on the two spheres treated as one body, and not of the resultant force on one only. I hope soon to find time to calculate the extra terms, and to lay them before the Society, but I wish to draw attention at once to the error there made.

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